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ERRATA IN No. CVIII, OCTOBER, 1874.

Page 273, line 22, *for* defer *read* demur.
„ 274, „ 1, „ of living the *qua*, *read* of living, *qua* the.
„ 275, „ 4 from bottom, *for* of the brain *read* to the brain.
„ 282, „ 19 „ *for* corrected *read* counted.
„ 284, „ 2, *for* monals *read* morals.
„ „ „ 23, „ internal *read* interrupted.

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BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

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Analytical and Critical Reviews.

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MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1875.

Analytical and Critical Reviews.

I.—The Limits of Unpaid Service.[1]

MUCH has been said of late years about the abuse of medical charity, and, as often happens, there has been a mixture of folly and wisdom in what has been said. Some persons who have spoken or written on the subject have betrayed their ignorance of the internal arrangements of our profession, and have advocated measures which were too sweeping to be practicable. Others, again, have professed to deny the existing evils altogether, and have been disposed to rest contented with the present state of things, and even to allow the abuses to go on growing with the growth of our population. But in the present review we shall aim at avoiding both these extremes. We hope to state the case fairly, without exaggeration, and we shall endeavour to propose only such remedies as are compatible with the present medical arrangements of the country. But we beg our readers to understand that, as becomes a medical review, we approach the subject from a professional stand-point. We shall not ignore general considerations, such as we might dwell upon if we were treating the matter from a national point of view; but we shall pass lightly over them,

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[1] 1. *Report of the London Hospital.*
2. *Report of the Royal Free Hospital* (*London*).
3. *Report of the Royal Ophthalmic Hospital, Moorfields.*
4. *Report of the Lock Hospital* (*London*).
5. *Report of the Hospital for Sick Children* (*London*).
6. *Report of the City Dispensary* (*London*).
7. *Report of the Chelsea Dispensary* (*London*).
8. *Report of the Royal Albert Hospital, Devonport.*
9. *Report of the Northampton Provident Dispensary.*
10. *Report of the Leamington Provident Dispensary.*
11. *Report of the Labourers' Self-aiding Medical Club of the Grantham Union.*

and shall put most prominently forward, and discuss at most length, those questions which affect us as medical men, and which have the most direct bearing upon the standing and reputation of the medical profession.

We have put these numerous reports at the head of this article to show (1) the evils of which we have to complain, as they are exhibited on a large scale, and (2) the various forms of the remedy that we venture to urge.

The reports of the several medical charities—free hospitals and dispensaries—show how enormous is the number of persons who annually seek advice and medicine from these institutions. Several writers have computed what is the total number of individuals who annually apply to the medical charities of the metropolis, and they tell us that it is over a million. We have ourselves gone carefully through the figures for the year 1873, and we make the total 1,288,085. This is altogether exclusive of the Poor Law and of a great number of private and semi-private institutions which publish no reports. Beyond the medical charity which can be estimated in figures there is a large amount which cannot be tabulated. We may, therefore, rest assured that the total at which we arrive by adding together the figures given in the various published reports is not an exaggerated one; but that, on the contrary, it represents only a proportion, a very large proportion, no doubt, but still only a proportion of the whole medical charity of the metropolis. And what is true of London is true also, though in a less degree, of the provincial towns and of the country at large. We may, therefore, safely conclude that a very large percentage of the community rely upon medical charity in time of sickness. In the metropolis this proportion amounts to something like a quarter.

This, then, is the first fact to which we call attention—the enormous number of persons who expect to receive their medical attendance and medicine at the expense of their neighbours, as a matter of charity. It needs no argument of ours to prove that this is a very undue number. In exceptional circumstances—for example, when famine devastates a country—a great part of the population may have to rely upon charity in one shape or another for the necessaries of life. But if such a state of things became chronic, we should think that it augured very ill for the prosperity of the people. Soup kitchens are excellent institutions on an emergency, but it would not be beneficial to have them always in operation for the supply of all comers. And the principle is the same with regard to medical charity. Sickness is not one of the necessities of life, and yet it may truly be said to be one of its neces-

sary accidents. It is a contingent event, but a contingent event which, speaking generally, is certain to come sooner or later. Is it wise, then, to rest contented with a state of things which permits so large a proportion of the population to rely upon the charitable help they can obtain from others in a matter which is, sooner or later, a practical certainty, almost as much a certainty as that another meal will be needed or another suit of clothes? It seems to us that no thoughtful person can acquiesce in a state of things which, under the name of charity, is in truth pauperising a large section of the community, and inducing them to depend, not upon their own prudence and forethought, but upon the aid they can derive from others.

The establishment of facts such as those referred to shows both the extent—the unreasonable extent—of this medical charity, and also the deteriorating effect that it has on the population. Those facts indicate the evils of which we have lately heard so much, and which afford a just ground for urging that the medical arrangements for the lower middle class should be in a great measure altered.

Now, how do these facts affect the medical profession? They affect us at many points. It is impossible that a large section of the community should get into the habit of expecting to receive gratuitous advice without our profession being touched by it in many ways. We shall mention what appear to us to be some of the most important.

Perhaps we ought at the outset to allude to that which affects, not ourselves only, but the whole community, and to state, first of all, that gratuitous work is often very indifferently done. No one will deny this who is familiar with the way in which patients are seen at crowded hospitals and dispensaries. If no more time is expended upon private patients than is proper and becoming, then what shall we say of the manner in which hospital patients are seen? The busiest men cannot see their private patients at home at the rate of ten an hour. But hospital patients are frequently seen at the rate of fifty an hour. It is true that private practice demands some little amenities which are not required in public practice. But still, after making all due allowance of this kind, we hold that one great evil of the present system is that patients do not receive the time and attention which their cases demand. In other words, they do not receive what the hospital professes to give them. The patients do not get the careful advice which they fancy they will gain by resorting to a large institution, and this last does not carry out the object for which its founders or governors have given their money. Such slipshod work as much of that

which is performed in the out-patient department of hospitals is a fraud both on poor and rich.

But how comes it that this work is so indifferently performed? In some cases there can be no doubt that it is on account of the vast number of the applicants. The physicians and surgeons are overwhelmed. A long day would not suffice to see the patients properly at the rate of twenty an hour, and so means are devised of seeing them at the rate of fifty an hour. Who can blame the medical man? He gives up two or three whole afternoons every week to this unpaid service. It would be unreasonable to expect him to give more. He must have time to earn his bread, and it is not wonderful that he should so hasten through his hospital work as to secure time for more remunerative labour.

This leads us to another point—a point which is of more general application, and which touches all who are engaged in practice, whether their *clientèle* be so large as to be overwhelming or not. It is this: unpaid labour, or labour which is all but unpaid, is sure in the long run to be badly done. In the ordinary affairs of life we all recognise and act upon this principle. It has become almost a proverb that unpaid service is worth just what it costs. Nothing can be got for nothing. But in our medical arrangements we proceed upon an opposite principle. We expect the doctors to be always ready to give their best advice, we expect that all who come and ask for it shall receive a valuable commodity, and all this is demanded from medical men, not occasionally, but constantly. Regularly, day by day, twice a week, or three times a week, as the case may be, from year's end to year's end, the physician or surgeon is expected to be at his post, bestowing his services upon all comers. Is it wonderful if this regular and constant gratuitous service, extending, it may be, over a quarter of a century—is it wonderful if it sometimes degenerates into a mere routine? Is it wonderful if it is occasionally badly done? Is it wonderful if it falls under the general law that unpaid service is worth just what it costs?

We have here assumed that the medical man gets nothing for his services at the hospitals and dispensaries which are supported by voluntary contributions. It may be replied that, though he receives no salary, he has the advantage of being connected with a public institution, and of the field of experience which this affords. Very true; we are not forgetful of these facts. But a man cannot live either upon his connection with a public institution, or upon the experience it affords him. They do not put money into his pocket. They do not put bread into his mouth, and we may well doubt whether they will always

secure the best of his energies. A certain amount of work, no doubt, he would willingly do out of pure benevolence—out of the same benevolent feelings which lead the public to give their money to support the hospitals—and for a certain amount of work he would, no doubt, consider the experience gained at the institution a fair equivalent. But it will probably be years before his connection with a hospital leads to any pecuniary return, if, indeed, it ever does so. Several sad cases which have occurred lately, and which must be fresh in the memory of our readers, show that men may be possessed of great ability, and may serve first-rate hospitals for a score of years, and yet not attain to a remunerative private practice. The lottery ticket proves a blank, and the medical man is ruined. The supposed advantages are poor remuneration for the regular, continuous, and responsible work that is required from dispensary and hospital doctors. It is contrary to the first principles of political economy to suppose that such arduous services are adequately recognised, as matters now stand; and if they are not adequately recognised, we may be sure that in the long run they will not be well performed.

Though we speak thus, and though we feel strongly that the work of seeing out-patients is not universally done as it should be and as it might be, yet we have repeatedly heard it said that the way in which it is now done, notwithstanding all the drawbacks, is highly creditable to the profession. In this opinion we quite agree. Nothing is further from our wish than to censure our brethren. We are only saying that they are human. We are only saying that they fall under the laws which rule all mankind in respect to the work by which they gain their livelihood. We sincerely hope that our efforts, coinciding as they do with a movement which is now going on all over the country, may tend to bring about changes in the mode of administering medical relief to the lower middle classes which would be advantageous alike to them, to the medical profession, and to the nation at large.

Intimately connected with the preceding objections to our present system of out-patient medical relief is another. Not only is the number of applicants out of all reasonable proportion to the population, and not only is an excessive burden of unpaid labour thrown upon the medical officers, but the patients themselves are in many instances such as ought not to be encouraged to resort to a charitable institution. We do not speak now of those cases in which gentlemen and ladies betake themselves to hospitals. We do not speak now of the gentlemen who leave their broughams at the corner of the street, or of the ladies who come habited in silk dresses and seal-skin

cloaks. Such cases do, no doubt, occur. Most of us have occasionally come across something of the kind. But these are quite exceptional, and it would certainly not be worth while to propose any great change in the arrangement of our out-patient departments in order to get rid of such gross cases of abuse. These are properly cases of *abuse*, and of an abuse so flagrant that it can hardly become very wide spread. What we allude to is the habitual *misuse* of the hospitals by a large number of the lower middle class, who certainly do not belong to that section of the community for whose benefit the hospitals were originally founded or are now carried on. This evil has, no doubt, grown up gradually, chiefly in consequence of the very faulty medical arrangements of the old Poor Law, and partly also in consequence of the pernicious habit,—one that even the first-rate hospitals and dispensaries have adopted, of advertising the number of their applicants as a means of exciting the sympathies of the public. But, however it may have arisen, the fact is indisputable that the hospitals are now used to a large extent by those for whom they were never intended. In other words, they are greatly misused.

In confirmation of this statement we may mention two or three particulars. Thus, Mr. W. H. Smith, M.P. for Westminster, has stated publicly that at one time he caused inquiries to be made into the condition of the out-patients at one of the large metropolitan hospitals, and he found that twenty per cent. gave false addresses;—a fact which looks very much as if they had something to conceal which would have told against them as applicants for the charity. Again, inquiries were made last year into the circumstances of 366 of the out-patients at the Queen's Hospital, Birmingham, and of this number 106 were considered unsuitable upon one ground or another. Again, a similar investigation was instituted at St. Mary's Hospital, Paddington, into the circumstances of 26 out-patients. The result showed that of these—

5 were cases of persons not suitable for hospital treatment; *i. e.* they were persons not at all in distress, and who could afford to pay a medical man;
12 were cases who could well afford to pay for the benefits of the provident dispensaries;
2 gave false addresses; and
7 were considered proper objects for the charity.

But no one who is familiar with dispensary and hospital practice will need to be convinced that many apply who are not "really poor," but who, on the contrary, are well enough off for their position in life. If even at a general hospital we stop to inquire into the wages and circumstances of our out-patients we shall soon have reason to doubt whether it is fair upon the

general practitioners of the neighbourhood to allow them to be gratuitously relieved. For example, is a nobleman's head gardener, suffering from stricture of the urethra, a suitable applicant? Is a hosier's assistant with a salary of £100 a year, though a married man with two children, to be allowed to attend for the cure of a few boils? Is a plumber's foreman, a single man, in regular work and earning thirty-five shillings a week, with a simple reducible hernia, a suitable applicant? Is a smith, suffering from a secondary syphilitic eruption, a young, unmarried man, also earning thirty-five shillings a week, a fit person to be admitted? We put forward these cases because they have come before us recently, and have led us to inquire, Is it fair upon the medical profession that such should be treated on the eleemosynary principle? Is it just that general practitioners, who have expended a large sum upon their medical education, and who are perfectly qualified to treat these cases, and whose charges the patients could well afford to pay —is it just that the general practitioners should be deprived of them? We say nothing here of the pauperising influence of the hospitals upon high-class working people, such as these. That is not our present subject. What we are here concerned with is the folly and injustice of the present system as it affects the profession. But if such examples as those given above are common at a general hospital, what shall we say of the special hospitals? Probably at some of those which are at the present time the most popular and have the highest reputation the instances of misuse would amount to at least fifty per cent. of the applicants. We say this advisedly, after having had good opportunities for forming an opinion. Now, if this is the truth or anything like the truth, how great an injustice is thereby inflicted upon the profession! But we have spoken only of strong cases—cases of single men earning regularly thirty-five shillings or more a week. But there are many besides these who frequent the out-patient rooms, many who are not earning quite so much, or who, if earning the same, have more claims upon them, who might fairly be expected under a better system to do something to help themselves. It has been shown by statistics that under such arrangements as we shall presently suggest those who are earning twenty-five shillings, twenty shillings, or even less, a week, find it for their advantage to contribute to their own medical relief. With such facts as these before us, and having regard to the excellent provision which is now made for the medical necessities of the pauper class, it is difficult to resist the conclusion that the system which prevails in our hospital out-patient department ought to be in a great degree altered, and that a large part of the work

which is now done for nothing ought to be placed on a remunerative footing.

This leads us to one more evil, and the last we shall mention, which the present system inflicts upon our profession. It is this. The excessive amount of unpaid labour has a tendency to depreciate the whole scale of our professional remuneration, and thereby to lower our social position. This, we think, must be clear to any one who reflects for a moment upon the subject. If we are so ready to give away the commodity in which we deal, and by the sale of which we earn our livelihood, namely, medical advice, the public naturally conclude it cannot be worth much after all. People are not usually so willing to give away that which they consider of value; and it is not unreasonable that when the public see medical men vieing with one another in setting up free hospitals and dispensaries, and offering their gratuitous services to all comers—it is not unreasonable that they should conclude that a medical man's advice is of very little account. But there is another way in which this excessive amount of "charitable" attendance cheapens our services and lowers the scale of our remuneration, and it is this—when no limit is put upon the grade of applicants at our hospitals and dispensaries, many of the lower-middle class find their way to them. But the lower-middle class are connected by many ties with the classes above them, *e.g.* as servants and masters, work-people and employers. And the master or the employer not unnaturally grudges paying a large fee for that which his butler or his foreman gets for nothing. He knows that his *employés* are well off for their station in life, and could afford to pay something in proportion to their income; and he asks, If the doctors are ready to treat them for nothing, why not the classes above them? It may seem to us absurd to argue in this way, but still there can be no doubt that it is frequently done, and that it is this which leads to our being so often asked to see patients without a fee, or to see them twice or thrice for a single fee. Thus, our willingness to exercise charity where charity is really needful has led, by want of strict limitation and control, to the whole scale of our professional remuneration being lowered and broken down. If we were somewhat more chary of giving our services indiscriminately, if we insisted upon the hospitals and dispensaries taking such precautions as would insure our being required to give our gratuitous services only to the really poor, it would have a very wholesome effect upon the entire scale of professional remuneration; and not only upon our remuneration, but also upon our social status. For it cannot be doubted that if our work were better paid, and if there were less haggling about fees, our profession would be

more esteemed by the public than it now is; and society would gradually come to set a higher value upon our services, and to believe that the article we have to sell is really worth the buying.

These, then, are some of the chief grounds why we say that great changes are needed in the mode of administering out-patient relief. The relief is inefficient, partly because the medical men are overwhelmed by numbers, and partly because the amount of unpaid labour is excessive; and this inordinate amount of unpaid work tends to cheapen the whole of our professional services, and to lower both our scale of remuneration and our social position.

The remedy which we would propose for this unsatisfactory state of things is a large extension of the provident system of medical relief. This system, as our readers are aware, has been in operation for nearly half a century; but during the last few years public attention has been specially called to it, and it has undergone a great development. This is the best proof we could offer of the soundness of the principle. It has stood the test of time, and increased consideration of the subject has only brought it the more into favour. We say the *principle* has stood the test of time, for there is, no doubt, room for considerable difference of opinion upon some points of detail, and it may hereafter be found necessary to vary the particular features of a provident institution according to the character of the population in different localities, the rate of wages, and so forth. But of this we feel sure that some means must be found of making the pence of the well-to-do poor contribute to the supply of their own medical relief, and that this can in no way be done so efficiently as upon the provident principle. The industrial classes of this country are rapidly rising both in social position and in political importance, so that they are better able to afford to pay than heretofore, and the very causes which have led to an amelioration in their condition have led also to an increase in the price of all the necessaries of life, so that the doctor is less able than before to offer gratuitous service. These two facts alone seem to us to show that there must be a change, and that some means must be found of making the working classes provide for themselves against the time of sickness. Lord Shaftesbury, in opening the Shaftesbury Park, stated, and the statement has been confirmed by others, that the aggregate receipts of the wage-earning classes in this country are not less than £400,000,000 per annum; and that of this sum at least one quarter is expended upon deleterious commodities, or upon things that could easily be dispensed with. If this is the case, we may reasonably demand that they should

do more for themselves than they now do; and, in particular, we may fairly expect that they should cease to depend entirely upon charity for their medical relief, and that they should in a great degree provide for themselves, at least in all ordinary ailments. And there is no way in which they can do this so easily and so satisfactorily as by associating themselves together in provident medical societies.

We need scarcely explain to our readers how provident dispensaries or provident sick societies are carried on. They are now so numerous that most medical men have had their attention called to them. Suffice it to say that each member makes a small but continuous payment from week to week, or from month to month, and that this entitles him to medical attendance and medicine when he is ill. The payment is usually about a penny or a penny halfpenny a week for each adult, and a halfpenny a week for each child; but it is seldom that more than four children in a family are charged for. These payments are so small that they are obviously within the reach of the great mass of the working classes, and where they can be induced to join in sufficient numbers the dispensaries are reasonably remunerative to the medical men who are connected with them.

Thus, at Northampton there were, in 1873, 12,820 members, and £1619 were divided among the three medical officers.

At Altrincham there were 2920 members, and £533 were divided among four medical officers.

At Camberwell (London) there were 5696 members, and £555 were divided among six medical officers.

At Haverstock Hill (London) there were 2326 members, and £320 were divided among three medical officers.

At Leamington there were 3436 enrolled members, and £336 were divided among the three medical officers.

In the Labourers' Self-aiding Medical Club, which comprises the parishes of the Grantham Union, there were 4577 enrolled members, and £689 were divided among nine medical officers.

At the Royal Albert Hospital, Devonport, an experiment is being tried which is well worthy of attention, and which may exercise a most important influence upon the hospital system of the country at large. There the out-patient department is itself on the provident footing, and the way in which it is spoken of in the last report is very encouraging. "The progress of the Provident Dispensary has been rapid—almost beyond expectation. In the first complete year of its working, ended on the 30th September, 1869, the receipts from all sources were in round figures £351; they are now increased to £538.

The committee are more than ever confirmed in their view of the advantages arising from the institution; there can be no doubt whatever that its principal merit is to establish and increase habits of providence and forethought among the working classes, but great relief is also afforded to the suffering poor. Several cases have been admitted as in-patients of the hospital on the recommendation of the junior surgeons—not as a matter of charity, but as a part of the relief purchased by their original subscriptions. In this way the members have the benefit of the superior appliances of the hospital, and the latter is a gainer as a medical school by being supplied with a succession of important and selected cases."

In all these provident institutions there is a charitable element. Donations in one form or another are given by the richer neighbours. But in time we may hope that they will become altogether self-supporting. It only needs that such institutions should be naturalised among our working people, so that they should enroll themselves in greater numbers, and then the provident sick societies would be able to stand firmly upon their own feet.

Perhaps in some parts of the country an extension of the club system may be found to supply their place. But, as a rule, we believe that provident institutions will be more advantageous than the benefit clubs, both to the poor themselves and to our profession; and this for several reasons. The clubs, for the most part, receive only men. They make no provision for the women and children. But these it is who are most frequently in the doctor's hands. And, again, the club doctor often finds himself at the mercy of the managing committee. He is forced to be content with the capitation fee they offer him, or they will introduce some one from a distance. And when the contract is once made, he has very little voice as to who are admitted members of the club, so that he may be obliged to visit for the small capitation fee persons who could well afford to pay his ordinary charges. But the provident dispensary, on the other hand, is arranged on such a scale of payments as will give the medical officers a reasonable return for their labours, and its benefits are limited to the grade for whom it is intended. Still, as we have said, a modification of the club system may be found best suited to some localities. Both benefit clubs and provident dispensaries stand on the principle of mutual assurance, and our preference for the one over the other is chiefly in matters of detail.

Any institution of this kind in which the interests of the medical man can be reasonably guaranteed, in which he has a fair voice in the management, and in which he receives an

adequate remuneration, seems to us to offer great advantages as a means of carrying on practice among the humbler classes of society. For example, the payments are all made in advance. There are no bad debts. And the payments are made, not to the doctor directly, but to the secretary. The medical man has no small bills to send out and to collect. Again, the patients—at least such of them as are well enough—are seen at the institution, and there also all the medicine is dispensed. These are no small advantages, as any one who is conversant with the harassing details of general practice among the lower middle class will readily allow. And the more medical men can be freed from such petty pecuniary matters the better standing will the profession generally enjoy in the eyes of the public. It is vain to raise the standard of medical education unless we raise also the income of the general practitioner, and the mode in which it is received. It is vain to adopt measures which necessarily increase the expense of training unless we offer men a better return for their outlay. It is vain to make men more cultivated, more refined, unless we rid them also of some of those accompaniments of poor practice which are intolerably irksome to gentlemen. The complaint is becoming general that the supply of well-qualified medical men is no longer adequate to the wants of the people in poor and densely populated districts. One reason, we have no doubt, of this deficiency is, that the men who now enter our profession are of a higher stamp than formerly, and practice as it is now carried on in such localities is distasteful to them. It would be a great gain if the whole medical treatment of the lower middle class could be conducted by means of provident sick societies. An attempt is now being made at Manchester to effect something of this kind. The whole city has been mapped out into provident dispensary districts, and the co-operation of many of the charities and a large proportion of the medical men has been secured. We believe that such a plan as this will do much to supply the alleged want, and to prevent the poor from falling into the hands of unqualified practitioners. It is only, as it appears to us, by the adoption of some such system that the poor can be saved from the pauperising influences of the medical charities, and that their small payments can be made subservient to the support of the medical profession, while at the same time the colleges and the universities pursue the admirable course upon which they have entered of raising and upholding the standard of scientific medical training. If such plans are put out of court, it will soon come to this—that the whole lower middle class will either be driven to the hospitals and free dispensaries, or

thrown into the hands of druggists, or, still worse, into the hands of quacks.

But, though we thus advocate provident dispensaries and sick societies, it must not be supposed that they can be successfully carried on without close supervision. The fact is that they are, in their turn, just as liable to be abused as the free hospitals, and require to be just as jealously guarded. Both the one and the other offer specially advantageous arrangements to limited classes of the community. But others of a higher and richer class are sure to try and avail themselves of them unless a very strict watch is kept. Hence it comes to this, that what is needed at free hospitals and dispensaries alike, in order to protect the general practitioner, is a systematic inquiry into the circumstances of each applicant in all cases except those of emergency and accident, in order that it may be ascertained whether he or she is a proper person to be admitted to the benefits of the charity. And similarly, when a person seeks admission to a provident dispensary or sick society, inquiry should be made by the committee to assure themselves that he belongs to the stratum of society for which the institution is intended. If at any time the committee have reason to know that a member has risen in the world, that his circumstances have become such as to elevate him above the class for whose benefit the provident dispensary is carried on, they are justified, after due notice, in striking him off the roll. To allow his name to remain on it when he is capable of paying the ordinary charges of a general practitioner would have a deteriorating effect upon himself, and would be an injustice to the medical profession at large. Without a strict supervision, such as this, it is impossible to carry on properly either a free or a provident charity. It may seem to some that such close supervision is inconsistent with the idea of charity, and that it quite takes the gloss off benevolence. But a little reflection will soon satisfy us that in such a densely populated country as ours, and in a highly complex state of society, we are in danger of doing more harm than good unless we give our alms, be they of what sort they may, with discrimination. And medical relief is no exception to this general rule.

To sum up. We ought upon every ground—in justice to ourselves, in justice to our patients, in justice to the community at large—we ought to insist upon strict limits being placed to the unpaid service which we are required to perform. We ought to insist upon the hospitals and dispensaries making some inquiry as to the social position and circumstances of those who apply for out-patient relief. We ought to insist

upon it that all those who can, without hardship, pay three halfpence a week should be referred to provident sick societies, and that those who can afford to pay the charges of a general practitioner should be directed to apply to one. This ought to be our programme, and there can be no doubt that if we were united in making these demands they would speedily be acceded to. By these means the total of out-patients would be reduced to a more manageable number, while a large proportion of those who were drafted off would be enrolled in a self-aiding system, which would pay adequate salaries to its medical officers. Nor let it be thought that these changes would impair the efficiency of the hospitals as places of medical education. Whether the proposals we have made were carried out by placing the out-patient department upon the provident footing, or by encouraging the establishment of provident sick societies in relation with the hospitals, by either plan the supply of acute cases for the wards would probably be more regular than it now is. This, as we have seen, has been proved by the experience of the Royal Albert Hospital at Devonport.

It is to our profession that the country looks for its medical arrangements, more especially to our profession as represented by the General Medical Council, by the Colleges of Physicians and Surgeons, and by other corporate bodies. Our hospitals and dispensaries are to a great degree—perhaps to too great a degree—managed by the laity. It is not to those benevolent gentlemen who sit on the weekly boards, nor to those charitable ladies who give their money for the support of the hospitals, that the country at large looks for the supply of its medical wants. It is to the medical profession itself. It is we who ought to decide what alterations are needed, and how they should be carried out. If we neglect to do this, we are falling short of the height of our dignity, and failing in the great public duty committed to our trust.

Now, we maintain that our public duty is, to see that the lower middle class is not pauperised in the matter of medical advice, and to insist upon it that the hospitals and dispensaries shall put strict limits to the gratuitous relief that they offer to out-patients, so that they shall no longer impose an unreasonable amount of unpaid labour upon their staff, or give the general practitioners of their neighbourhood any just cause of complaint. And further, we ought to see that the country is well supplied with provident sick societies, so that those upon whom the hospitals shut their doors may have no lack of opportunities for obtaining good medical attendance on terms which they can well afford out of their weekly wages. If these

things were done, it would be much for the benefit of the nation, for it would tend in one important particular greatly to strengthen the moral fibre of the nine or ten millions who form the wage-earning class of this country, namely, by encouraging in them habits of self-reliance, forethought, and prudence. It wonld also tend to improve both the pecuniary remuneration and the social status of our profession, by transforming a great part of the work which is now done gratuitously into remunerative service. The work itself would be better done than at present, and when the pay of the profession was better, and was received in a more dignified manner, a higher class of men would be attracted towards it, scientific and professional education might be carried to a still higher point, and thus the changes we have advocated would be advantageous in all their bearings.

II.—Military Hygiene.[1]

For many years the 'Treatise on Hygiene' by Michel Lévy was used in France alike by civil and military practitioners. In its thoroughness it showed Lévy's German origin, for he was from Alsace; in its clearness and method it proved his French training. It was a work worthy of the man who for so many years had the direction of the great school of Val de Grâce, and who even under the pressure of a mortal disease boldly kept to his post when the storm of a fatal war enveloped France, and was happy in being permitted to live long enough to serve her through the worst time of her great misfortune. It was a work which must always hold a leading place in the history of preventive medicine, and its vast stores of learning will long be a mine where his successors will dig for the abundant materials Lévy's patient industry stored up.

But when the great German war was over, and the medical service of the French army, shattered to pieces like the army itself, began to reconstruct itself, the work of Lévy was not found to meet the demands of the time. It was too general in its method and too extended in its scope. A work was required which should deal exclusively and exhaustively with the army alone, and should show alike to the Government and to the officers of the French army how indispensable to the efficiency of the army are the lessons of military hygiene.

We think that few men could have accomplished the task

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[1] *Traité d'Hygiène Militaire.* Par G. Morache, Médecin-Major de première classe; Professeur agrégé au Val de Grâce. Paris, 1874. Pp. 1040.

better than the author whose work we are about to review. He is both a scientific physician and a practical soldier; he is deeply read in his special subject and versed in its various applications. His style possesses the usual clearness and methodical arrangement of the French school, and though, like so many of his countrymen, he occasionally loses time and space by details which belong to other subjects, and a knowledge of which should be assumed, he is not such a sinner in this way as many others, and this slight fault is as nothing compared with the mass of practical and useful information his work contains.

The first book, containing 247 closely printed pages, is occupied with the organization and recruiting of armies. Although not fully expressed in any one passage, though hinted at in more than one, a ruling thought pervades and animates the whole of this chapter. Morache attempts to weigh with impartiality the aptitude of the French nation to furnish an army which may compete with Germany. That thought seems ever before him, although he would probably not confess it, and the gravity of the question has led to great care and moderation in his conclusions. He traces up the various recruiting rules in France, especially from the year 1666, when Louvois may be said to have created the French army, until the years 1872 and 1873, when under the pressure of her great calamity France has been called to an almost general armament. The law of July, 1872, has imposed an obligatory military service on all citizens, and has abolished the replacements and substitutions which under the Second Empire so fatally weakened the army both physically and morally. The conscription takes place on the 1st July in each year, and includes all men who in that year completed their twentieth year; voluntary engagements for one or five years may, however, be entered into by men of eighteen years. The law of July, 1873, completed the army organization. By this last law France is divided into eighteen regions and subdivisions, each of which is to furnish a corps d'armée, consisting of two divisions of infantry, a brigade of cavalry, a brigade of artillery, a battalion of engineers, a squadron of equipage-train, as well as a general staff and the various necessary auxiliary services. Each of the corps d'armée is supplied in its own territory with every requisite for entering at once on a campaign, and thus the great concentration and the consequent inevitable confusion which so delayed the French army in 1870 will be prevented. The general officer at the head of the corps d'armée commands every military force in his district, not only the active army, but the territorial and reserve, and all the

military services of supply. He has under him two chiefs of the staff, one with the active army, who marches with the troops when mobilised; one with the territorial army, or, as we should call it, the reserve, who is charged at all times with the recruiting, with the cavalry remounts, with the hospitals, and all the territorial duties. The staffs of the engineers, the artillery, and all the administrative services, including the medical department, are also divided into these two sections, viz. one with the active army, the other with the territorial.

Independent, however, of these "états-major," the commander of the corps d'armée has near him and under his orders the officers concerned with the direction and movement of the administrative and the medical services. In other words, the vicious system of the French Intendance, which created an *imperium in imperio*, is destroyed, and the responsibility of the general in command extends, as it ought to do, over the whole of his army.

What number of men will France then furnish when her system is complete? In the active army in time of war she will have, after accounting for the army in Algeria, for all sick or imprisoned soldiers, and for all the so-called non-combatant services, a force of 1,090,000 men. Behind this will be the territorial army of 1,208,156 men.

Such is the immense force which has roused Germany to increased exertion, and which, if Providence does not prevent such madness, will repeat, it may be on the very plains where Attila marshalled his innumerable hosts, but with increased elements of horror, the dreadful conflict, not of armies, but of nations armed.

Now as to the units of this army, the men who, willingly or not, must obey the dictates of national hatred or apprehension. Morache passes in review the various rules in the European armies of the height, weight, and girth of chest of the recruits. As regards France itself, he comes to the conclusion that the physical aptitude of the French population for war is not diminishing, but is sensibly improving (p. 103). This is based on a consideration of the exclusions from the army for defect of stature and infirmities during twenty-five years ending with 1868. The proportion of rejections for deficient stature—*i. e.* for men under the old regulation height of 1832 (1·56 m. or 61·4 English inches)—has fallen from 84·1 per 1000 of recruits in 1844 to 50·6 per 1000 in 1868, and for the last fifteen years the improvement has been progressive. So also the exemptions from infirmities have also fallen. It appears, then, that the French conscripts are somewhat increasing in height as a rule, though Morache considers that the number of tall men is not

augmenting. On comparing the French and other nations it is evident that the French conscripts are less in stature than the Teutonic and Sclavonic nations. Morache gives the following table (to which we have added the heights in English inches) to show the influence of race:

Minimum stature of infantry recruits in the principal armies.

		Height in mètres.		Height in English inches.
Germanic races	Prussia	1·621	...	63·8
	Northern America	1·600	...	63·0
	England[1]	1·600	...	63·0
	Sweden	1·608	...	63·3
	Baden	1·570	...	61·8
Mixed Celtic races	France	1·540	...	60·6
	Italy	1·560	...	61·4
	Belgium	1·570	...	61·8
	Spain	1·560	...	61·4
Germano-Sclavonic races	Austria	1·553	...	61·1

The law of 1872 lowered the regulation height in France from 1·56 m. or 61·4 inches to 1·54 m. or 60·6 inches, *i. e.* by two thirds of an English inch. Morache considers this correct, as he regards height as an attribute of race rather than as a sign of strength and endurance. He would not, therefore, consider that the minimum height of the French conscript being three inches lower than the Prussian is a sign of inferiority in the former; it is merely a question of race. On the question of the connection between height and health he is opposed to the views of Quetelet and Villermé.

From the height Morache proceeds to the question of weight and girth of chest; we will not follow him through his careful analysis, but merely remark that the present rule in France (law of July, 1873) is that the minimum thoracic girth (mean of inspiration and expiration) should be 0·784 m. (or 30·866 English inches), which is 0·014 m. more than the half-height. In Austria and Russia the rule is that chest girth shall surpass the half-height by 0·02 m., or nearly three quarters of an English inch more than the half-height. The rules Morache would himself lay down for French soldiers are thus given (p. 119):

"1. Among healthy robust individuals the height, the weight, and the thoracic perimeter augment proportionably, although the ratio is not constant.

"2. Every centimètre (= ·3937 English inch) of augmentation

[1] Morache seems to have put the English minimum too low. As far as we know it has never been down to 63 inches in the last eighteen years.

of height brings with it, among healthy individuals, an absolute augmentation of weight and of thoracic perimeter, while the ratio of this perimeter to the half-height lessens.

"3. The most favorable conditions of military aptitude occur with a height of 1·60 m. to 1·70 m. (= 63·8 to 66·9 English inches), corresponding to a thoracic perimeter of 0·861 m. to 0·924 m. (= 33·9 and 37·38 English inches).

"4. Above the height of 1·70 m., when the half-height and the perimeter are too near, there is the chance that the chest will be narrow, the respirations shallow, and a predisposition to thoracic affection may exist.

"5. No man should be accepted in the army whose thoracic circumference does not exceed the half-height by at least 2 centimètres ($\frac{3}{4}$ English inch nearly) if the individual is 1·60 m. in height (63·8 inches), and 3 centimètres ($1\frac{2}{10}$ inches) if the person is under 1·60m. in height.

It will be seen from these remarks how very important Morache considers the girth of the chest. A large chest, in fact, indicates for the most part, not only free respiration (that is, the capacity for taking in more oxygen to develop force), but larger bones and thoracic muscles.

With respect to weight, Morache proposes a graduated scale of height and weight. The minimum height being 1·54 m. (60·6 English inches), he proposes a minimum weight of 57 kil. or 125·6 lbs. avoir., and for every centimètre of height (= $\frac{4}{10}$th of an inch nearly) he would increase the weight from 370 to 400 grammes. In other words, an increase of half an English inch should give an increase of about 1 lb. avoir. in weight.

Probably all our readers will pause now to ask how our own nation stands when judged by these rules. This is a point not very easy to answer, for the English is a voluntary and not a conscripted army, and besides at the present moment an experiment is being tried which puts another difficulty in the way of the comparison.

The English army, as it existed ten or even six years ago, was an army of large men as compared with the population. In 1862 the minimum height for infantry was 66 inches, or 5 inches more than in the French army; in 1864 it was reduced to 65 inches. For cavalry it was from 66 for the light to 68 inches for the heavy troops. This gave an army of fine powerful men, who contrasted favorably with all other European armies. Unfortunately there was nothing behind this army; when these splendid troops were sent on service and wasted to skeleton battalions, as in the Crimea in a few months, there was no reserve to fill up the thinned ranks with equally

good material. The boasted army was in reality a pageant merely; its body was of brass, its feet of clay. To all who look at European politics and see that with every effort to keep at peace England may be drawn into the whirlpool, it has become a certainty that somehow a reserve must be made. Whether the plan which Lord Cardwell devised, and which is now being carried out, will answer or not, time only can show, but it is a *boná fide* attempt at a reserve. Men are enlisted for six years, and then are draughted into the reserve. The very essence of the plan is to train men quickly, and to pass as many as possible through the ranks. But under the conditions of the labour market it has been found impossible to obtain men of the old height in sufficient numbers. They are not to be had, and consequently the standard of height has been lowered and the limit of age has not always been insisted on, or has not rigorously been inquired into, so that many mere boys, whose growth is not ended, are now taken into the army. Hence, in the ranks of the English infantry are seen now a great number of very youthful and of comparatively small men, which is exceedingly displeasing to the old officers accustomed to a different style of man. But their objection is, after all, superficial; the healthy army life improves the growth and power of these youths; they grow into fine vigorous men,[1] and then, if they can be kept in sight, will form a reserve which, like the Prussian Landwehr, will give us a second or reserve force of finer and more powerful men than the first army. When in an infantry regiment a number of very young men are seen, this is only a proof that the system is working and that soldiers are being manufactured to be kept in readiness for any great emergency. But this plan for forming a reserve naturally renders the comparison of height between the English and French soldier less favorable to the former than it was.

At present men are enlisted for the infantry of the line at eighteen up to twenty-five years of age, and with a minimum height of $64\frac{1}{2}$ inches. It is the opinion of Dr. Leith Adams, the present able medical recruiting officer at the London headquarters, that we have nearly reached the lowest possible standard for the Anglo-Saxon race, and that a height below 64 inches would be accompanied by so insufficient muscular development that the man would never be made an efficient soldier. But it does not appear that at present we have gone below the efficient height.

We have called Lord Cardwell's scheme for a reserve a

[1] The boy of 18 years and of $64\frac{1}{2}$ or 65 inches in height will, as a rule, if well fed, gain three inches in the next five or six years, and will eventually stand at 67 or 68 inches. If, however, he be 23 or 25 years of age he will grow no more.

bonâ fide one; and coupled with his plans for connecting the line, militia, and volunteers, and his formation of county brigades, it seems to us also well judged. But opinions differ very widely on this point, as may be seen from the discussion which followed Dr. Leith Adams's very able paper on recruiting at the Royal United Service Institution in February, 1874.[1]

It was doubted whether we shall get the men; whether when drafted into the reserve we shall keep them; whether, in the attempt to add to our voluntary enlistment the advantages which a conscription gives, we have not taken away many of the inducements which formerly took men into the army and kept them there. Some officers think that the principle of short service has not been carried far enough; that there should be short-service soldiers of two years, passing then into the reserve for eight years, or enlisting at option into a long service, which would meet our Indian and colonial requirements, and which should carry with it increase of pay for every term of five years, and attractive pensions at the end of the service. Others may think that the long service of twelve and twenty-one years, with better pensions, must be returned to, and that the true reserve must be found in a compulsory county militia service, embracing all ranks and connected, as it will be in Lord Cardwell's plan, with the line and volunteers.

All these are matters of great difficulty, and must be decided by statesmen and soldiers; but this much seems clear—that so far we are not going wrong with our selection of men, although any further lessening of height would be a hazardous experiment.

The following are the heights, weights, and measurements Dr. Adams would like to see in the English army:

Type of	Height.	Chest girth.	Weight.	Girth of thigh.	Girth of forearm.
	inches	inches	lbs.	inches	inches
Infantry . . .	67 or 68	36	145 to 150	21	11
Rifleman . . .	64	35	130	20	11
Gunner	69	37	160	21	11
Heavy dragoon .	69	36	140	19	10
Lancer	69	35	140	16	10
Hussar	66	35	132	19	10

Of course men of this style cannot always be obtained, though even now our splendid artillery and cavalry regi-

[1] "The Recruiting Question," by Surgeon-Major Leith Adams; 'Journal of the Royal United Service Institution,' vol. xviii, 1874, p. 55.

ments probably come very close to the type. But as already said, the number of young growing lads would at present probably bring the actual infantry measurements much below Dr. Adams's typical soldier.

Another question connected with height and weight has been much debated. Which arm of the service should have the biggest men? In our service the heavy cavalry have the largest men, the light cavalry and the artillery come next, and the infantry are the smallest. This is the general rule in Europe. Morache, however, questions its propriety; he considers that by far the heaviest work falls on the infantry; and that—certain corps, such as the cuirassiers and artillery excepted—the largest men should be foot soldiers. For the light cavalry, he says, if a man be big enough to saddle and clean his horse, and robust enough to manage his sabre, he can make a good cavalry man.[1]

This view has, however, not been adopted in France, as the following table will show.

Minimum height of the different services in the French army (law of 1872), *age* 20.

	Mètres.		English inches.
Infantry	1·54	...	60·6
Cuirassiers	1·70	...	66·9
Dragoons	1·66	...	65·4
Chasseurs (à cheval) . .	1·63	...	64·2
Hussars	1·63	...	64·2
Artillery	1·67	...	65·8
Pontonniers . . .	1·67	...	65·8
Engineers	1·66	...	65·4
Equipage trains . . .	1·64	...	64·6
Troops of administration .	1·54	...	60·6
Hospital corps . . .	1·54	...	60·6

The question is certainly one which demands more attention than it has received in England; the work demanded from infantry is increasing; longer and more rapid marches are made, and the men ought to be very strong and enduring on their feet. The work of the cavalry is also increasing, for more ground has to be traversed, but here the great point must be to relieve the horse of weight. We think that Morache's view is the correct one, and that all light cavalry should be small men and be lightly accoutred.

Whether heavy cavalry will exist or not in the army of the future it would be rash to say; it is certain, however, that in

[1] He writes this with the impression on his mind of the late changes in the French light cavalry equipment. There are now no lancers in the French army; dragoons, hussars, and chasseurs are alike equipped with sabre, carbine, and revolver, and will all perform the same service, viz. to explore the country and to guide the army, and to act more by rapidity than by weight.

the present English army all the cavalry are practically heavy; there is very little difference between the heavy dragoon and the lancer.

After discussing these points Morache proceeds to the very important national question of the "infirmities which render men unfit for the military service." It is, of course, only in the nations with conscription that such a subject can be properly investigated, and it is one advantage of a conscription that a government can gain a very good knowledge of the physical condition of its population. In April, 1873, the "Conseil de Santé des Armées" published a list of the infirmities which exempt from military service in France; of these there are 100 headings, and in addition there are twenty-four more infirmities which unfit for the active army, although not for the territorial.

We will not dwell on this chapter, because the real interest of the physical investigation of the French population which the law of universal military service of 1872 renders necessary, will only commence after several years' accumulation of materials. The value of the future materials will be easily seen from the following blank form which is ordered to be filled up every year in France for every youth of twenty years.

RECRUTEMENT DE LA CLASSE 18...
CORPS D'ARMÉE.

Département............................ Canton..............................
No. obtenu au tirage...........

Nom et prénoms.....................
Date de la naissance ....
Enfant légitime ....légitime.....................naturel.

Lieu de naissance . {
De l'apelé ....département de.........................
De son père .... „ ....
De sa mère .... „ ....
Sa famille paternelle est originaire du département de.........
„ maternelle „ „ ....

Célibatoire.......................marié ou veuf.................avec.................enfants.

Instruction . . {
Nulle......................................
Primaire ....
Secondaire ....
Supérieure ....
Grades universitaires ....

Profession.............................

Résultat de l'Examen d'Aptitude Physique au Service Militaire.

Subi le..........................18...
Devant le ....

1°. Taille . . . . . 1 mètre.........millimètres.
2°. Poids . . . . kilogr.........grammes.

3°. Circonférence thoracique . {
Avant l'inspiration.........millimètres.
Après l'inspiration.........millimètres.
Distance des mamelons ....millimètres.

4°. Aspect général et développement musculaire .
- Vigoureux............
- Ordinaire ....
- Chétif
  - Congénital..................................
  - Pathologique suite de ....
  - Convalescent de ....
  - Suite de misère..........................

5°. Dentition . . . .
- Saine..................
- Mauvaise ....
- ....dents absentes.

6°. Tête, diamètres maximums
- Antero-posterieur.........millimètres.
- Transverse.........millimètres.

7°. Face . . . . .
- Hauteur.........millimètres.
- Largeur.........millimètres.

8°. Cheveux . . . .
- Couleur...............................
- Abondants ....
- Rares ....
- Absents par suite de.............

9°. Vision . . . .
- Couleur des yeux..................
- Distance de la vision normale lit à.........centimètres.

10°. Couleur de la peau..................

Accepté pour le service .
- Actif...............
- Auxiliare en raison de...............

Ajourné . . . .
- Une première fois pour ....
- Une seconde fois pour...............

Exempté pour...............................
Dispensè comme...........................
En sursis d'appel comme................
Incorporé dans le.........regiment d.........(brigade......division.........corps).

The most paternal government could not inquire more carefully into its subjects' appearance, and we think Dr. Beddoe will be delighted to see that he will soon be able to know the colour of the eyes and hair of all the twenty-year-old inhabitants of France. We pity, however, the unhappy officials who have got to fill up this long list, and still more the unhappy recruits who are thus tenderly looked over, merely to be made food for powder. The only persons who will be happy are the statisticians, who will obtain innumerable rows of figures to add up, and who will be able to tell the size of the head and the number of the teeth of every young man in France.

We pass on now to the second book, which is on the habitations of the soldier, and consists of 312 pages. Barracks, fortresses, hospitals, camps, tents, &c., form the subject of this chapter. All these points are considered with great fulness and in the most enlightened manner. As the subject of buildings has been, however, so much discussed in this country, we turn to what is said on the subject of tents. In the last war the Germans carried no tents, and it is well known that their officers do not believe that any will be provided in a future war; we were therefore very curious to see what might

be the French opinion in this respect. Morache quotes the dictum of the first Napoleon, which was adverse to tents, and dissents from it as an absolute rule, but he does not go further into the question; we infer, however, from the context that he believes tents will be used in future wars. He condemns the French *tente-abri*, which was introduced in Algeria in 1850, and considers that though it was useful in Algeria, and may be so in Europe in the summer, its power of protection does not counterbalance the great objection of its weight (1·820 kilogrammes or 4·2 pounds avoir.).

Although, therefore, the *tente-abri* is still the regulation tent in the French army, we infer that opinion is against it, and certainly its great weight is a most serious objection in these times of rapid movements. And yet it gives some cover at the time when the soldier most needs it, viz. at the end of a long march, when the larger tents may not arrive for hours; at that critical time on a cold rainy afternoon a shelter-tent is a real boon, and we are inclined to doubt whether Morache has arrived at a right conclusion; we think a modification, and not an abolition, would be the proper solution. If the *tente-abri* is done away with in the French army it is not yet certain what form and shape the large tents will be; the two tents now used for the men for camps which are relatively permanent are the double-folded tent, or *tente Taconet*, holding sixteen men, and the conical single-poled tent, or *tente Turque*, which is also for sixteen men. This last is considered the best; it is heavier than our bell tent (126 pounds avoir. as against 70 pounds), but much more airy; its great weight will probably cause it to be modified before it is adopted for a moving army.

On the whole, it would seem that it is yet doubtful what the French will do in this matter of tents; at present they adhere to the old plan and the army will carry its *tentes-abri*.

After laying down some excellent rules for the hygiene of tents Morache proceeds to the subject of "baraques" or huts.[1]

He notices that the great military hygienic writer Vaidy recommended, in 1818, the use of "baraques" in preference to tents for fixed camps, and thus directed attention to what Morache justly considers "one of the most important subjects

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[1] In France the permanent barracks are "casernes;" the "baraques" are simple one-storied buildings of wood, or brick, or concrete (or pisé, *i.e.* earth concrete). In Germany the term barrack is now also used in the French sense to denote these sheds. The word "baraques" does not, therefore, exactly correspond with our English word barrack. We mention this as a little confusion has sometimes arisen from the "baraques" being thought to be identical with the "casernes."

of military hygiene." It is only since the Crimean war that their great utility, not only as barracks for troops, but also as hospitals, both military and civil, has been recognised.

In the war of 1870-71 they were largely used by both French and Germans, and since the end of the war a large part of the French army has been kept in camps with "baraques." Many of these erected during the war were not very well made, but others were very good. At Bourges the baraques are 90 feet (English) long, 26 wide, nearly 10 feet to the eaves, 20 feet to the ridge; each hut is divided into five compartments, each holding twelve men, and the cubic space is 530 feet per man; the gables are masonry; the walls are of wood externally, but inside is a wall of single brick, between which and the wood is a space; only the officers' huts have ceilings; the floor rests on joists, which are elevated no less than three feet above the ground by a rubble foundation. These appear to be the best yet built in France, since the war, but the "baraques" built for the German army of occupation were, Morache thinks, even better; they were designed, he says, by a German military commission, which was presided over by a Prussian medical officer, and this commission imposed its will on the architects and builders. Morache makes a remark on this point which must not be omitted.

> "This fact," he says, "viz. giving the presidency of the commission to army surgeons, contrasts singularly with the mode of acting in the French army, where the doctors are not consulted by regulation when it is a question of constructing habitations for the troops, but only when all is finished, and when errors the most capital and often irremediable have already been committed."

Let us hope that the publication of this book will vindicate for our French brethren their proper position in their army, and that they will no longer be called in to prescribe when the patient is dead.

In speaking of the furniture of the "baraques" Morache refers in terms of praise to a "bed-hammock," invented by a civil engineer, M. Maurice, and tried at Meudon in 1872 with success; it is, in fact, like our sea cots, and is suspended by cords; one end of the cot rests on a strong rod or lath (tringle) nailed to the wall, and the other end is supported on a movable table which supports two cots; during the day the cot is pulled up against the wall by a simple arrangement. It is important to remember, however, that in our own army hammocks were formerly used at some of the foreign stations, and led to excessive overcrowding; the introduction of bedsteads was of incalculable value as their width obliged more lateral space to

be given; if hammocks are ever used again in any form, their use must be guarded by the most stringent regulations.

With respect to the hygiene of camps, Morache gives, as usual, very clear and useful rules; he discusses at some length the system of latrines, and concludes that, as far as possible (*i. e.* in camps of position), the system of "fosses mobiles" should be used, so as to allow no accumulation. He speaks favorably of the earth system and of the "Goux" plan.

Morache speaks in very strong terms of the favorable influence of the camps on the health of the troops in 1871 and 1872 round Paris, as well as formerly in the old camp at Chalons. The two great maladies of the French army, typhoid fever and consumption, "pass in the camps into the second line;" the diseases are chiefly "telluric" *i. e.* paroxysmal fevers or diarrhœa, or are rheumatismal; "but typhoid fever is rare, as well as chest diseases, and especially tuberculosis" (p. 553).

We pass on now to the third book, on the clothing and equipment of the soldier, which occupies seventy pages. With regard to the colour of garments, a table is given (p. 567) to show the visibleness of different colours on different coloured soils and backgrounds, and at different times of the day. White is the most easily seen at 300 mètres, and grey and brown the least so; dark blue is rather more easily seen, and red still more so, though it is not quite so visible as white in most lights. In certain lights red is visible at 300 mètres when all other colours are not seen—for example, at sunset, when green, dark blue, white, grey, and brown, are all invisible. At 600 mètres red is much less visible than white and not much more so than green and blue; at this distance grey and brown are in some cases not seen at all, as, for example, when they are backed by earthworks or rocks. This table appears carefully done, and ought to lead to renewed experiments on the part of our military authorities. Morache considers it absolutely condemnatory of the red trousers of the French, and it appears, indeed, that the "pantalon rouge" has been merely retained to encourage the madder industry of Alsace, and this is a consideration which cannot weigh for a moment with that of the safety of the soldier in action.

With respect to the head dress of the soldier, the French now wear a low shako, only 11 centimètres high in front (= $4\frac{1}{3}$ inches) and 14 (= $5\frac{1}{2}$ inches) behind, with a moderately inclined peak. Morache does not think it so good as the German Landwehr shako, though better than the spiked helmet of the German active army. In the French cavalry

the shako is extremely various in form and material, but at present an experiment is being tried with a very low cavalry shako for the light troops; the cuirassers will still carry the metal helmet, but much lighter than the one used before the war, which was so heavy that it produced neuralgias from pressure, and early baldness. It is earnestly to be hoped that the heavy, ugly, hot, and inconvenient shako lately introduced into the rifle and fusileer regiments and for the engineers of our own army may soon be discarded again, for nothing worse has been contrived for a long time.

To go from the head to the feet. Morache discusses with great care the shoeing of the soldier. Marshall Niel said in the Legislative Assembly, in 1868, that "the boots have for the infantry the importance that horses have for the cavalry," and this is true "au pied de la lettre." In France, as in England, the pattern of a boot is still not decided, and for the twentieth time a commission was called in 1873 to decide on this point. On the whole, Morache prefers boots to shoes; lays down the usual rules as to shape and fittings, and recommends the leather to be greased with the following:—Mutton fat, four parts; lard, two parts; yellow wax, olive oil, and turpentine, of each one part.[1] The new boot is washed, dried, then covered with a thin layer of the preparation, exposed to the sun or before a fire (care being taken not to burn the leather), and then rubbed well with a piece of flannel until the fat is well worked into the leather; the leather preserves its suppleness.

We turned with interest to the questions of the weight carried by the infantry and cavalry soldiers respectively. The French foot soldier carries—including his clothes, armament, tente-abri, and part of the provisions (4 days)—a total weight of 32·918 kil. or 72½ lbs. av. If all the provisions are carried, and if the tente-abri is wet, the weight reaches 35 kil. or 77 lbs. av. This enormous weight simply renders the operations of war impossible, and Morache proposes changes which would reduce it to a total of 25·746 kil. or 56 lbs. av. This smaller weight would be 6 lbs. lighter than the German soldier carries, but nearly as much more than the weight borne by our own troops. Happily the English soldier is now less burdened than any other army, but it is quite possible to lessen the weight still more without loss of efficiency.

The weights of the light French cavalry (dragoons) equipment are thus given:

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[1] Formula prepared by Surgeon-Major Tomaine, and said to answer very well.

	Kilogrammes.
On the horseman	17·220
Harness	20·900
Packet borne in front of man . .	8·073
Packet borne behind man . . .	24·791
Mean weight of man	65·000
Total charge on horse . .	135·984

Even in a light cavalry regiment the French horse carries, therefore, no less than 300 lbs. av. or more than 21 stone. This is 2½ stone more than the horses in our hussar regiments carry, and even our weight is thought to be too great by some officers.

With the same fearlessness which characterises his work throughout, Morache condemns strongly the French infantry knapsack, and contrasts it unfavorably with the new English valise equipment. He describes and figures the latter, and then says (p. 625)—

"Experiments have been made in France to determine *de visu* the value of the English valise. They have been conclusive. After a march of 32 kilomètres (20 miles nearly) the majority of the soldiers experienced no fatigue; they agreed with the officers appointed to direct the experiment in saying 'that the chest was completely free; that they did not find with the valise that adherence of the sac to the body which fatigues and harasses the man and impedes his respiration; that, finally, the equilibrium is perfectly maintained by means of the two pouches and the ball-bag, so that the man can preserve his vertical position, the centre of gravity being always on the straight line passing through the centre.'" "It is to be hoped," adds Morache, "that these results will not be sterile for the French army."

The fourth book, containing 258 pages, is occupied with the subject of the food of the soldier.

As our review is already too long we propose to reserve this chapter until we receive the next part of the great work of Roth and Lex on military hygiene, which is to contain the chapter on food. We pass then to the fifth chapter, on "Military Life," which occupies 70 pages and includes, of course, the important subject of marching. Morache justly calls the calculations of marches *i. e.*, the length of time a body of men will take to cover a certain distance, one of the most difficult problems in the art of war. The actual rapidity of the march itself is only a small part of the problem; the time necessary for the formation of the column, for the rear to arrive at the end of the march at the point the head has reached,[1] for

[1] Marching on a single road, the entire length of road occupied by a corps d'armée (30,000 men) is more than fifteen miles, even in the closest order practicable.

the movement of the convoy, and for the maintenance of intervals so as to avoid the stoppages which are followed by accelerations (in technical phrase "les à-coups), has to be calculated. When the movement of infantry and horse is simultaneous, when the country has to be searched or is difficult to get over, the extent and rapidity of marches are equally modified; then, again, heat, dust, want of water, all tell against the soldier, and rain, head winds, ice and snow, greatly delay him. Morache says the tacticiens consider 20 kilomètres (12·4 miles) as an excellent average for a corps d'armée; Colonel Lerval[1] would raise this to 24 kilomètres (14·9 miles). The mean of all the marches from 1796 to 1815 made by the French army, of those made in Italy in 1859, and those of Austria in the Prussian war of 1866, give a mean of 21·89 kilométres (13·4 miles) In 1870-71 some portions of French corps made daily marches of 32 to 36 kilomètres (20 to 22 miles), but this was felt to be an immense effort.

It may be thought all these particulars are rather for the military than the medical officer, but like many other army problems both classes of officers ought to study them. A march of twenty miles carried on day after day by loaded men is a severe trial for health, and must be provided for by well-calculated solid and liquid food; meals at certain times, rest at proper intervals, and by the most careful study of boots and equipments, which may affect the marching power of the men. Then the influence of the weather, heat, cold, has to be considered, and as far as possible guarded against. Well may Morache say that marches offer a problem of no little complexity both in a purely military and an hygienic sense.

Morache has also a few words on transport by railway. To the soldier this is an irksome transport when the distance is long, on account of the overcrowding; the men cannot sleep or change their positions, so that after a long railway journey they are often not in a condition for a forced march. Railways, however, are convenient for feeding troops, as the food can be prepared at intervals along the line and be in readiness when the train arrives.

We have left ourselves no space to refer to the other points in the life of the soldier to which Morache alludes—his intellectual life, his habits, and his vices. In point of fact this part of the subject is not given in much detail, nor do we notice anything of special moment.

The sixth book gives an account of the sanitary institutions and establishments of the army, *i. e.*, disinfecting rules and appa-

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1 'Conference sur le Marche d'un Corps d'Armée.' Paris, 1870.

ratus; prophylaxis of certain special maladies (smallpox, syphilis, &c.); infirmaries, hospitals (fixed, movable); and in an appendix are a few pages on the morbidity and mortality of the French army. The last page of this appendix brings us to the end of the book at page 1029.

Although the work is thus a large one, we can truly say it is not too long. We lay it down with the feeling that it is written by a thorough master of his subject and by a man of singular judgment and common sense. There is also one feature about it which to us has been very pleasing and has greatly enhanced the pleasure we have had in reviewing it. It is just and even generous towards the writers of other countries, and although Morache is a Frenchman to the backbone, and evidently feels deeply the reverse of the late war, he shows none of that petty temper towards the Germans which some of his countrymen have exhibited. He makes use of and refers in terms of high commendation to the masterly German work of Roth and Lex on military hygiene, and he has allowed no national prejudice to betray him into the least shade of discourtesy; there is something in the style of his book at certain parts at once dignified and touching; it reminds us of the graceful chivalrous bearing which in older times the French are supposed to have showed to their enemies.

The tone of this book makes us feel, indeed, what a strong bond a common profession and a common object creates among the medical services of all armies. It makes us hope that if war does come again there will be between the French and German surgeons, no national hostility, but merely a generous rivalry, seeking to bear to friend and foe alike that priceless help which in the time of his sorest need comes to the sick or wounded soldier, and represents alone Christianity amidst the heathenism of war.

III.—Recent Psychological Doctrines.

(*Continued from Vol. LIV, p.* 292.)

In our last number we examined some recent psychological works of a scientific and positive character. We have now to take up those on our list which are speculative and philosophical, and which we classed accordingly as they are logical and mystical. Of the philosophical kind two are the works of philosophers by profession, exhibiting, however, differences both as to method and objects. Professor Bain discusses according to his own method that fundamental problem of the relation of

Mind and Body which Dr. Carpenter believes he has solved, and Mr. Lewes the great "problem of Life and Mind" according to his. The titles of the works indicate a radical difference in method, and *à priori* in favour of Mr. Lewes's, since, as we have shown in criticising Dr. Carpenter's views, the problems which Professor Bain undertakes to elucidate involve the philosophy of biology, which he, however, excludes.

We take Mr. Lewes's work first, as dealing specially with method and as the result of a life-long experience. How he was led by a process of evolution to the mature views he gives us is interesting as a fact in positive mental science. It is interesting also as elucidating the defects in the methods which both Professor Bain and Dr. Carpenter have followed. Mr. Lewes states that—

"The work of which this is the first volume has been many years in preparation; indeed, its origin may be said to go so far back as 1836, when, with the rashness of ambitious youth, I planned a treatise on the Philosophy of the Mind, in which the doctrines of Reid, Stewart, and Brown, were to be physiologically interpreted. In 1837 I gave a course of lectures on the subject in Fox's Chapel, Finsbury. The scheme was abandoned, partly because of a growing dissatisfaction with the doctrines of the Scotch school, and partly, perhaps, from a misgiving as to my physiological knowledge. Other studies and other labours occupied me until 1860, when I believed that my researches into the nervous system had placed in my hands a clue through the labyrinth of mental phenomena, and, misled by the plausible supposition that the complex phenomena in them might be better interpreted by approaching them through the simpler phenomena in animals, I began to collect materials for a work on Animal Physiology. This also proved to be premature. Rightly to understand the mental condition of animals we must first gain a clear vision of the fundamental processes in man, since, obviously, it is only through our knowledgè of these processes in ourselves that we can interpret the manifestations of similar processes in them; and here we are hampered by the anthropomorphic tendency which leads us to assign exclusively human motives to animal actions." (*Preface.*)

Mr. Lewes then informs us how, in 1862, he began the investigation of the physiological mechanism of Feeling and Thought, and sought assistance to his researches in "Anatomy, Physiology, Pathology, Insanity, and the Science of Language." He commenced with the intention of writing a series of essays treating on certain problems of Life and Mind; he ended with two new schemes—the one to determine the fundamental indications necessary to the constitution of Psychology, the other to indicate how the method of Metaphysics should be constituted a scientific method. It is this latter scheme which chiefly

occupies the volume before us, and which may be said to aim at the development of a modern *Novum Organon* in the form of "a systematic introduction to the philosophy of science." But, Mr. Lewes observes, "what was intended merely as a preparation for psychology discloses itself as the foundation of a creed."

The history Mr. Lewes gives of the results of his cerebral evolution and concurrent mental development will be recognised as not unlike their own by those who have freely thought out the great problems of Life and Mind. In the evolution of ideas, as in that of the organism proper, there is an order, and it would be both useful and interesting to work it out. We shall consider, however, the chief result at which Mr. Lewes aims, viz. the combination of metaphysic with positive science, since this is a fundamental characteristic of his method.

If the method of metaphysics be that of the speculative and scholastic schools, and which entirely exclude biology and physiology, then its repudiation by scientific inquirers is inevitable. But there are degrees in this respect. Besides, since the differentiation of states of consciousness is necessary for the correlative differentiation of states of brain, in so far as the metaphysical method has effected this differentiation its results may be accepted. There are, however, physicians who think that to differentiate the simplest manifestations of consciousness, such as, *e. g.*, the corporeal sensations of heat and cold and the like, is to be "half metaphysical," and therefore speculative and unsound. In short, the slightest approach in medicine to metaphysics is denounced. Mr. Lewes endeavours, therefore, to meet current prejudices, and to place metaphysics in a new and better light. He says:

"The word Metaphysics is a very old one, and in the course of its history has indicated many very different things. To the vulgar it now stands for whatever is speculative, subtle, abstract, remote from ordinary apprehension, and the pursuit of its inquiries is secretly regarded as an eccentricity, or even a mild form of insanity. To the cultivated it sometimes means scholastic ontology, sometimes psychology pursued independently of biology, and sometimes, though more rarely, the highest generalizations of physic. * * * It may be preserved if we separate it from its method as that which comes after physics [according to Aristotle] and embraces the ultimate generalizations of research. It thus becomes a term for the science of the most general conceptions." (p. 15).

From this point of view modern science is full of metaphysics, and those who most stoutly repudiate it most energetically adopt it. No better illustration of this could be given than Professor Tyndall's discourse "On the Scientific Uses of Imagination." Hence Mr. Lewes truly remarks:

"Few researches can be conducted in any one line of inquiry without sooner or later abutting on some metaphysical problem, were it only that of force, matter, or cause; and since science will not, and metaphysics cannot, solve it, the result is a patchwork of demonstration and speculation very pitiable to contemplate. Look where we will, unless we choose to overlook all that we do not understand we are mostly confronted with a meshwork of fact and fiction, observation curiously precise, besides traditions painfully absurd—a compound of sunlight and mist. Thus, in various writings we come upon Laws which compel phenomena to obey their prescription—Plans and Archetypal Ideas which shape the course of events, and give forms and functions to organisms—Forces playing about like sprites, and Atoms that are at once extraordinarily indivisible and infinitely divisible—Bodies acting where they are not, and Non-Being (pure space) endowed with physical properties, among others that of resistance (since forces, in spite of their alleged independence of matter, are supposed to be diminished by the spaces they traverse)—these and many analogous phantoms, more or less credited, too frequently hover amid phenomena and convert speculation into what Hegel, in another connection, sarcastically calls 'a true witches' circle.' Why is this? Mainly because men of science are generally trained either to ignore all metaphysical questions, or to regard them as 'mysteries which must be accepted.'" (p. 9).

Maintaining both that experience is essential to knowledge and that there is a region of inquiry which transcends the limits of experience, Mr. Lewes would name the former *empirical*, the latter the *metempirical,* region. Physics and metaphysics would then include all in the empirical region, and "deal with things and their relations as these are known to us, and as they are believed to exist in the universe. *Metempirics* sweeps out of this region in search of the *otherness* of things; seeking to behold things, not as they are in our universe—not as they are to us—it substitutes for the ideal constructions of science the ideal constructions of the imagination." Such a metempiric is Serjeant Cox.

In these views the man of culture can readily recognise old notions under new terms. In acquiring knowledge man aims at two objects—the knowledge of external nature, and the knowledge of his own nature. With Bacon, Coleridge, and others, the understanding is that faculty by which a knowledge of the former is obtained, the reason by which we acquire a knowledge of the latter. As a fact of positive science, both kinds of faculties combine in all seekings after knowledge, and reason is but a higher evolution of the understanding. Considered out of relation to brain-work, Mr. Lewes's doctrines belong to the old philosophy. From the newer point of view,

whatever method be pursued, the results are all referable to brain-functions differently exercised in different men, for there can be no knowledge and no thought whatever independently of brain-work. There must, in fact, be a certain evolution of brain, not only to attain to the power of comprehending the subtle and comprehensive inductions of science, but even to take hold of the first step in any metaphysics and in all philosophy—the knowledge of the me distinct from the not me. This is shown, not only in the history of the development of each individual mind, from infancy onwards, but of that of the human mind as manifest in races. The distance in this respect between the understanding of the aborigines of the hills of India and the reasoning power of the Brahmins and Bhuddists of the plains, who have sounded the thinkable depths of all philosophy, is much greater than that between the animal *homo* in his most degraded form and the civilised ape.

But what are the facts of experience considered as subjects of scientific inquiry, according to the method of Mr. Lewes? What is empirical, what metempirical? He will not deny that our knowledge of atoms and of energies, of evolution and reversion, of the great laws of morphology, is not direct, or that the facts are far beyond immediate observation. Nevertheless chemistry, with its atomic theories, kinetics, kinematics, and physics in general, and, although last not least, biology, have all attained a most remarkable development in modern times. Yet the hypothesis of the atomic constitution of matter is of prehistoric antiquity; nay, Mr. Lewes's own division of knowledge into the empirical and metempirical is only an ancient notion redeveloped. The old *apeiron*, commonly translated "the Infinite," denoted that which transcended experience, just as *empeiria* denoted knowledge founded on or within the range of experience. In truth, all systems of philosophy, being the result of evolutions of the human brain on the common basis of certain primary facts of consciousness, must necessarily have principles and facts in common, although having specific differences accordingly as one primary fact predominates more than another evolutionally. This being so, the inductions of biology and the conclusions of biological philosophy as to evolution of brain must be made available to the elucidation of scientific methods.

To give one or two illustrations. How do we arrive at the notion of an atom in the contradictory senses which Mr. Lewes has correctly indicated? Thus; by experience we can divide and subdivide a piece of matter until it is no longer divisible; in thought or metempirically (metaphysically) we

can further divide and subdivide *ad infinitum.* By applying mathematics and experiment to a solution of the question of the infinite divisibility thus raised, Sir Wm. Thompson has deduced that atoms are finite, and of a size somewhere between the 250,000,000th and the 500,000,000th (five hundred millionth) of an inch thick. Further, calling in the help of the imagination metempirically, we obtain more definite notions of the size of atoms by imagining how big the molecules of water are. According to Sir W. Thompson, if a drop of water were enlarged to the size of the earth, and proportionably therewith each of its constituent molecules, it would appear to be like a heap of things of a size between that of small shot and a cricket ball. All this is, of course, a product of pure thought. From time immemorial to the present men have theorised in this way. In the philosophy of Herbert Spencer and other atomic physicists we have only a reproduction of Lucretius and Democritus with a difference.

It may be said that mathematics is an exact science, and wholly unlike metaphysics; but it is only exact because the signs of the notions which mathematical signs represent are exactly and definitely representative, which abstract terms are not. Having its solid foundation in number, we have to inquire how the fundamental fact of number originated as an experience. Could this be otherwise that in the fundamental fact of experience that we both see and feel we are one—an *individuum*—an atomic thinking organism? It is thus, presumably, that I and 1 bear the same sign visually. From this point of view the Ego of the metaphysician correlates a law of life. Nor is it otherwise with the sciences of motion: what primary experience have we of motion beyond that of the power we possess to move bodies? An historical inquiry into the development of words by which the cause of motion is denoted would trace back all knowledge to this primary fact of experience. All cosmogonies, whether ancient or modern, and all philosophical systems, depend on this primary fact of experience—that a man can put forth force so as to move bodies. A professor (Tait), eminent alike as a mathematician and a physicist, teaches us that the whole of the sciences of force and motion are included in this definition of Force—that when the velocity or direction of a body changes, or both change, "we naturally," he says, "attribute this to some extraneous cause. This cause * * * of whatever nature it be, is known by the general name of force."[1] The uses of the words "naturally" and "nature" are noteworthy here; by naturally is meant

[1] 'Dynamics of a Particle,' chap. i.

intuitively, by intuitions are meant fundamental conditions or elements of knowledge that come to us congenitally and independently of experience, and which have been recognised otherwise causally as "innate ideas"—instincts, &c. Biologically, they are due to properties of brain which come to us evolutionally. It is upon a like fundamental element of knowledge and fact of experience that the whole of reflective or introspective philosophy rests: we are conscious—*Cogito, ergo sum.* In Mr. Taylor's 'Primitive Culture' we may read how this fundamental fact has evolved into numerous hypotheses, theories, and systems, of philosophy, religion, and morals, varying with the varying extent of knowledge, but all of which can be classified under the general term *Animism,* because founded on the deduction that consciousness is due to a thing distinct from the body—"separable and separate" in Hamilton's words, and named soul, psyche, pneuma, anima, spirit, ghost, &c.

But another fact of experience is implied in the deduction that there is body, and still another that consciousness is intimately associated with *living* bodies. This may be said to come by experiment rather than by observation, for all the thinking conceivable would not suffice to assure a man that his thinking depended on his body, much less on his brains. But from the moment men found they could render others or be rendered themselves unconscious by a blow on the head experimental and somatic philosophy began. In thinking-out, therefore, by the natural method, what is the order of nature in any particular direction, every thinker must fundamentally come to the same conclusion. And so the true principles of all philosophies must be the same.

What, then, it will be asked, leads to such widely different views in philosophy? Setting errors of method aside, they arise chiefly from the different meanings attached to the words used to denote the generalisations arrived at, and more especially to the names by which generalisations as to Cause and Order are denoted. Mr. Lewes affords an instructive illustration of this point. Quoting Newton's well-known denunciation of hypotheses, he remarks that in the very passage which follows "he (Newton) has no hesitation in propounding a view which in these days must startle the most speculative by its wildness" (p. 52). The wildness is only in the use of the word "spirit" to denote a cause; substitute the word "energy" and Newton's hypothesis is a remarkable anticipation of modern scientific research. We quote it thus amended:

"And now we might add something concerning a most subtle

Energy which pervades and lies hid in all gross bodies; by the force and action of which *Energy* the particles of bodies mutually attract one another at near distances, and cohere if contiguous; and electric bodies operate to greater distances, as well repelling as attracting the neighbouring corpuscles; and light is emitted, reflected, refracted, inflected, and heats bodies; and all sensation is excited, and the members of animal bodies move at the command of the will, namely, by the vibrations of [caused by] this *Energy* mutually propagated along the solid filaments of the nerves from the outward organs of sense to the brain, and from the brain into the muscles. But these are things that cannot be explained in few words, nor are we furnished with that sufficiency of experiments which is required to an accurate determination and demonstration of the laws by which electric and elastic *energy* operates."

Here, then, is a thought-out hypothesis, which with the word "spirit" instead of "energy" startled Mr. Lewes, at least, by its wildness, but which, when a modern term is used to denote the general cause of motion, distinctly marks out the modern hypotheses of the correlation of the physical, vital, and mental forces, and that in language as lucid as has ever been penned by moderns.

We take another illustration of this kind from Newton, which Mr. Lewes quotes, without, we think, seeing how the ideas originate metempirically.

"In the preface to the *Principia* he [Newton] says—'For all the difficulty of philosophy seems to consist in this—*from the phenomena of motion to investigate the forces of Nature, and then from these forces to demonstrate the other phenomena.*'" * * *

What Newton thus outlined as to method is now being carried out by physical research. It is now possible to apply kinetics and kinematics to biology and physiology, and through these to psychology, and so a positive mental science will be developed.

We could extend illustrations on this and other questions raised by Mr. Lewes in his very interesting and valuable work; but what of the creed of which it is the foundation? the reader may impatiently ask. We subjoin the only exposition we can find; by its negations it indicates that, broadly, Mr. Lewes is an optimist, a modified Comtist, and an evolutionist. After some remarks on the present relations of science and religion, Mr. Lewes says:

"Other thinkers—and I follow these—consider that Religion will continue to regulate the evolution [of Humanity]; but that to do this in the coming ages it must occupy a position similar to the one it occupied in the past, and express the highest thought of the time, as that thought widens with the ever-growing experience. It must

not attempt to imprison the mind in formulas which no longer contain the whole of positive knowledge. It must not attempt to force on our acceptance, as explanations of the universe, dogmas which were originally the childish guesses of barbarian tribes. It must no longer present a conception of the world and physical laws, or of man and moral laws, which has any other basis than that of scientific induction. * * * Instead of proclaiming the nothingness of this life, the worthlessness of human love, and the imbecility of the human mind, it will proclaim the supreme importance of this life, the supreme value of human love, and the grandeur of human intellect. Those who entertain this hope and this view of a Religion founded on Science, expressing at each stage what is known of the world and of man, believe—and I share in the belief—that the present antagonism will rapidly merge in energetic co-operation." (p. 3.)

Briefly, Mr. Lewes is a modified Comtist as to method, and, as an optimist, believes in a philosophical millenium. His faith is truly "metempirical," as, indeed, all faiths must be; and is not a new faith, inasmuch as the belief in a distant age of perfection is prehistoric. But what say Science and Experience in verification? Like many moderns, an ancient philosopher "gave his heart to seek and search out by *weis*dom, concerning all things that are done under heaven," but thereby he discovered the futility, even to the philosopher, of human knowledge: "For in much wisdom is much grief, and he that increaseth knowledge increaseth sorrow."

Although we have no such high aspirations as Mr. Lewes, still faith in human progress is helpful, if not hopeful, and is certainly infinitely better in the interests of suffering humanity than the "epicurean stye" of John Stuart Mill, of which he left the age so sad a picture in his autobiography. And surely it is something in advance so to inquire into the order of nature as to deliver the human mind from the terrible miseries inflicted by ignorance and superstition. A religion, to be worth anything, must afford at least solace; and a religion founded on science surely serves to this end.

After all, however, belief, like philosophy, is fundamentally a question of brain-work; and whether it be optimism or pessimism will depend in part, at least, on the nutrient energy of the brains of the believer. A useful illustration of this fact is afforded by the experience of J. S. Mill as detailed by himself with that youthful rashness to which Mr. Lewes confesses, and which is so common at puberty in males. Mill, at the age of twenty, entertained the notion that he was to be the reformer of the world. But under conditions of defective brain-nutrition, such as too often leads on to incurable dementia in young persons of the same age, he awoke from his optimism and his

foolish pride as from a dream. He had, in short, a Hamlet-like melancholia, of which it may be at least said, "that way madness lies." The attack began in autumn, 1826, and during the succeeding months of winter Mill reasoned with himself as to the causes of his condition, and blamed his education "in analytic habits of thought" by his father. It was this which he believed had led him to that state in which he had no delight in virtue or the general good, but also just as little in anything else. The fountains of vanity and ambition were dried up. He had no love for music or nature, nothing but a dry, heavy dejection. He thought the lines in Coleridge's 'Dejection' exactly described his case; but Hamlet's description is more to the purpose, for Stuart Mill meditated on the propriety of suicide. He frequently asked himself if he could, or if he was bound to go on living, when life must be passed in this manner. That brain-nutrition was defective at this time is made certain by what he says as to his memory. "Of four years' continual speaking," he observes, "at that society [a debating society], this is the only year of which I remember next to nothing." He does not seem to have had any suspicion that his state was due to physical causes, themselves linked with a partial and one-sided development of his brain. "Advice, if I had known where to seek it," he observes, "would have been most precious. The words of Macbeth [to the physician] often occurred to my thoughts,[1] but there was no one on whom I could build the faintest hope of such assistance." His father with his "analytic psychology" was the last person to whom he could look for help. "Everything convinced me that he had no knowledge of any such mental state as I was suffering from, and that even if he could be made to understand it he was not the physician who could heal it. The sufferer seems to have taken the advice of the physician to Macbeth literally, "Therein the patient must minister to himself;" and after undergoing months of preventible and even dangerous brain-disorder, the reading of a tender domestic scene in Marmontel's 'Mémoires' moved him to tears; and the man who, if we were to conclude from his autobiography, never had a mother to love and reverence, was thus relieved from his mental misery. No better illustration of the uselessness of the philosophical method for the business of daily

[1] "Can'st thou not minister to a mind diseased;
Pluck from the memory a rooted sorrow;
Raze out the written troubles of the brain,
And, with some sweet oblivious antidote,
Cleanse the stuff'd bosom of that perilous stuff
Which weighs upon the heart?"

life could be given, or of the fallacy of the hopes entertained by enthusiasts of a philosophical millenium.

Professor Bain's work is one of the International series, written evidently to order; it is little more than a brief compendium of facts and theories as they have occurred to a mind trained essentially in the scholastic or speculative method. Hence it is that the mixture of facts in physiology with the deductions of speculative philosophy are incongruous when discussing the essentials of his problem; in truth, the problem is too big for his little book and his restricted method. After stating the question, Professor Bain adduces facts to prove "that the connection of Mind and Body is not occasional or partial, but thorough-going and complete," so that all kinds of conscious states without any restriction whatever are dependent upon concomitant changes in the nervous tissue, which, after H. Spencer, is described as a "nervous shock." In all this he is a true physiologist.

But when we come to examine more carefully what meaning the author attaches to general or abstract terms it is found that, like Dr. Carpenter, he uses them, especially as to causation, in the sense and with the ambiguity of speculative psychology. For instance, he remarks, "There is no example of two agents so closely united as mind and body without some mutual interference or adaptation." In what sense, we ask, is the word agent used here? and how is mind an agent and body an agent? If the various states of consciousness be due to certain conditions of the nerve-tissue named "shock," what part has "mind" in it? He remarks, "Most decisive of all under this head [of proof] is the wide experience of the insane. Among the chief causes of insanity must be reckoned excessive drafts on the mind—as, for example, long and severe mental exertion, and sudden mental shocks, usually of disaster and misfortune, but occasionally even of joy." (p. 13). This explanation might be fairly understood to be merely metaphorical, and that Professor Bain meant, perhaps, to say nothing more than this: insanity is said to be caused by so-called mental shocks; but insanity is a mental condition due to disease of the brain, and the mental shocks are truly "nervous shocks;" therefore it is the nervous shocks which cause the disorder of the brain-functions on which insanity depends. If this be his meaning why did he not say "drafts" on the brain, or on brain-power or energy, instead of "on the mind"? But evidently Professor Bain means to affirm with speculative psychology that the states of consciousness cause the morbid cerebral states, for he affirms that violent emotions are among the causes of paralysis, which is a disease of the nerves or

nerve-centres. So, also, he is of opinion that the emotions cause the muscular acts which are the signs of those states of consciousness, and that the intensity of the feeling is necessarily proportionate to the intensity of the movements. Now, all this is after the method of the reflective philosophy, and not that of scientific psychology. It is common to the whole of this school of psychology, with which Stuart Mill and Lewes must be classed. This theory of causation, as has been shown, has equally involved the "mental physiology" of Dr. Carpenter, although less widely than in others of the psycho-physiological school.

It is right to state, however, that practically Professor Bain holds to the theory that consciousness is not a cause of mental states, and corrects his own errors. Doubtless, it may be said that pain, emotions, the attention, imagination, and will, cause bodily changes in the same sense that we say the sun rises; to the sense of vision it appears to come up above the horizon, to our sense of feeling pain seems to cause cries. But as facts of science and of true causation both statements are equally erroneous.

It is, however, in the exclusion of biology and of the philosophy of evolution from his method that Professor Bain shows most clearly the results of his scholastic training. His "general laws of alliance of Mind and Body" have no root whatever in the laws of life. Take, as an example, the "Law of diffusion."

"When an impression is accompanied with Feeling, the aroused [nervous] currents *diffuse* themselves freely over the brain, leading to a general agitation of the moving organs, as well as affecting the viscera.

"*Illustrative contrast.*—The so-called reflex actions (breathing, swallowing, &c.) are commonly said to have no feeling; at the same time they are accomplished in a limited circuit or channel.

"*Note of explanation.*—It is not meant that every fibre and cell can be affected at one moment, but that a spreading wave is produced sufficient to agitate the body at large." (p. 52.)

Here we have a hint at the physics of neurology and mind, but the theory is even more metaphysical than "metempirical;" it represents no reality. In all his generalisations, in truth, Professor Bain betrays a want of scientific knowledge; there is little biology, little physiology, less physics, still less pathology. The relations of chemical affinity to cerebro-mental states are of great practical importance, since all the so-called "stimulants" are chemical compounds. Professor Bain prefers to substitute a "Principle of Stimulation" for molecular energy. He says:

"The last point that I will advert to is the obscure subject of narcotic stimulants—alcohol, tea, tobacco, opium, and the rest. These operate a very little way, if at all, in giving new vitality; they draw upon our vitality, even till it is much below par, postponing the feeling of depression till another day. It is probable that the influence of narcotics is complicated, and not the same for all. We may safely say, respecting them, that they are the extreme instance of the principle of Stimulation, as contrasted with the principle of vital conservatism; they are the large consumers, not the producers, of vitality; they expend our stock of power in nerve-electricity in a higher degree and with a more dangerous licence than the ordinary stimulants of the senses." (p. 72).

This is very little better physiology or philosophy than the learned Serjeant Cox's theories of the "psychic Force." There is, strictly speaking, no science—only the jargon. So also with the histology of the brain, which has a prominent place in the book; the author enters into an elaborate estimate of the number of "acquisitions" of knowledge a man can store up in his brain-cells. But if we deal with molecular physics we must deal rather with the constituent atoms or molecules of the cells. To explain, therefore, the *modus operandi* of stimulants it is necessary to look a little deeper into causation. What, for example, is the molecular state of the brain in "nervous shock"? and how will drugs help the sufferer? Like all other matter in its ultimate constitution, that of the brain is molecular or atomic. It follows, therefore, that the condition following on "shock" is a molecular or atomic disturbance due to physical impulses amongst the atoms. So that the theory of cells is not available to solve the problem, inasmuch as cells are no more the ultimate elements involved than are crystals in chemical compounds. Brain-cells are, in fact, only the forms which combinations of molecules or atoms assume in living things, such as is seen in primordial cells. They are endowed with distinctive properties, which, however, are rarely distinguishable. Further, the elementary bodies which enter into the chemical composition of these molecules are not unknown, and it is established that the changes in them which coincide with conscious states are chemical. Morphia, for example, which is itself the result of evolutional, or living chemical, affinities as manifested in a plant, coming in contact with cerebral molecules, excites the molecular conditions upon which various well-known pleasurable states of imagination and feeling depend; in especial, it has also the valuable property of modifying the conditions which cause both mental and bodily pain, and so relieving. In like manner as to chloroform and other drugs. Hence, to the physician this department of

psychology is of serious practical importance; and not less so to the moralist, for some of, if not all, these chemical compounds, when taken unwisely, either to relieve pain or to induce pleasure, may and do act so injuriously on the molecular constitution of the brains that the intellectual and moral nature is degraded. This is well known to be the fact as to opium and various intoxicants which contain alcohol; but bitters, like hop, wormwood, and strychnine, are also injurious. Such facts, in scientific and practical psychology, obviously bear directly on the science of morals and on moral philosophy. Professor Bain has to teach the clergy of Scotland; might he not more validly show the importance of scientific psychology to pulpit instruction, and the prevention of drunkenness and crime? The whole population can be best taught the folly of putting an enemy into their mouths to steal away their brains through the clergy; but if the latter are blind leaders of the blind, it is on undeniable authority we can affirm that both will fall into the ditch, as, indeed, too often happens.

Professor Bain's little work is, undoubtedly, in the right direction. In the interests of positive mental science it is to be hoped that he will develop his method to the exclusion (or at least correction) of all that is merely speculative.

We have now to examine the mystic class of books, and by mystic we mean all that in thought deals with the wonderful, surprising, thaumaturgic, and supernatural; all that, if not above and beyond the natural order of things, is, at least, deeply hidden from inquiry, and is occult in the literal meaning of the term.

The mystical in this broad sense of the word is widely comprehensive and may be made to include all knowledge comprised under animism, from Mr. Woodward's triune system and the psychic force of the spiritualists to Dr. Carpenter's "self-determining power" of the will. The latter offers, in truth, in another form the hypothesis of a "Psychic Force." Mr. Serjeant Cox has already enlisted the doctrine of "unconscious cerebration" in its favour. His next step is easy, for he needs only to identify this "self-determining power of the will," as exercised by the individual or "Ego" on himself, with that which mesmerists and spiritualists affirm is exercised on others and at a distance, and the hypothesis is complete.

The mystic school in theology is the orthodox school, and Dr. Carpenter has joined it—in spirit at least. This is shown, not only by his use of scholastic and theological terms, and especially of the word materialism in the vulgar meaning of the controversial theologian, but also most conclusively by his

method. His proof of the immaterial nature and separate existence of his "self-determining power" is eminently dogmatic and equally unscientific. A "positive conviction" that it exists is to him proof sufficient that it does exist, provided it be held by a "right-minded [orthodox] man, who does not trouble himself with speculation," that is to say, with thinking about the truth and accuracy of his convictions, and who is, in fact, right-minded theologically for not doing this. Dr. Carpenter, in short, gives in his adhesion to the scholastic theologians of Rome and to the philosophical teachings of Archbishop Manning. The following is genuinely orthodox both in phrase and substance:

"But these phenomena [of the "materialists"] are not to be looked at to the exclusion of the facts of our own internal Consciousness. In reducing the Thinking Man [all capital letters are in the original] to the level of 'a puppet that moves according as its strings are pulled,' the Materialistic Philosopher places himself in complete antagonism to the positive conviction which—like that of the existence of an External World—is felt by every right-minded Man who does not trouble himself by speculating upon the matter, that *he really does possess a self-determining power*, which can rise above all the promptings of Suggestion, and can, *within certain limits* (§ 25), mould external circumstances to its own requirements, instead of being completely subjugated by them." (p. 5.)

And in an approving note Dr. Carpenter refers to an essay by Archbishop Manning in the 'Contemporary Review' of February, 1871, as follows:

"The Writer entirely agrees with Archbishop Manning, in maintaining that we have exactly the same evidence of the existence of this *self-determining power within ourselves*, that we have of the existence of a *material world outside ourselves*. For, however intimate may be the practical correlation between Mind and Brain (§ 11, 12)—and Archbishop Manning seems disposed to go as far as the Writer in recognising this intimacy—'there is still [quoting Dr. Manning] another faculty, and more than this, another Agent distinct from the thinking brain. * * * That we are conscious of Thought and Will is a fact of our internal experience. It is a fact also of the universal experience of all men; this is an immediate and intuitive truth of absolute certainty,'" &c. (pp. 5, 6.)

Now, it is doubtful whether all men entertain these metaphysical notions. Doubtless the most uncultured savage discovers that he can often do by the use of his limbs what he wishes, and he *infers* from that and other facts of experience that there is something in his body distinct therefrom. On this all theories whatever of animism are founded. The question, however, is—not as to the fact of feeling, but—as to the

accuracy of inference. Upon a like inference, in truth, the whole of the mystic and supernatural is founded, and which, whether as manifested in barbarous peoples or amongst the highly cultured Asiatics and Greeks, led to the wildest theories of theology, biology, and cosmogony. A household instance will serve to show the fallacy of Dr. Carpenter's method. There was a time when all were certain that the sun moved through space, for do we not see it move? And certainly, as a fact of sensation, nothing is more certain; when, however, men took the trouble to speculate about the fact of feeling it was found that not the sun but the earth moved, and that fact has been established as a truth in spite of archbishops, popes, and cardinals making the contrary into a dogma not to be doubted by "right-minded men."

A more instinctive warning as to the danger of relying upon feelings, intuitions, and positive convictions, because it is drawn from the domain of science and philosophy, is afforded by the opposition which Leibnitz (unquestionably one of the greatest thinkers of his time) gave to the researches of Newton into the existence and laws of gravity. So absolutely unconscious are men of the force of the earth's attraction on the erect position they maintain, and on their freedom of locomotion, that Newton's demonstrations fell upon minds wholly unfitted by so-called experience to appreciate their value. In particular, Leibnitz, relying on his "positive convictions," distinctly repudiated Newton's experimental researches as being speculative. "I am a great friend of experimental philosophy," he patronisingly remarked, "but Newton deviates much from it when he pretends that all matter is heavy, or that each particle of matter attracts every other particle."[1]

What man, indeed, trusting to his "positive convictions" and to his "common sense," could reasonably admit that there is a pressure of fourteen pounds on each square inch of his body? These positive convictions are, in truth, simply the correlation of brain-states, so that if a "positive conviction," irrespective of inquiry into its nature and origin, could be admitted as undeniable evidence of the separate existence of a power or a thing, the hallucinated inmates of lunatic asylums must be held to be the best and surest sources of knowledge, for their positiveness is a chief element in their convictions. Being insane, Dr. Carpenter may say their will is in abeyance and their "convictions" worthless, but that is not the fact in his sense of the phrase; on the contrary, the "self-determining power" they possess (due to the strength of their convictions)

[1] 'Life of Sir I. Newton,' by Sir David Brewster, vol. ii, p. 60.

is the proximate cause of their seclusion. In like manner Dr. Carpenter maintains that the power of the will is suspended in dreams, and that mesmerised persons, however exalted their faculties, when in that state are like dreamers, "mere thinking automata, puppets pulled by directing strings" (p. 6). Still we ask, Can there be a more "positive conviction" than that which arises in the minds of all "right-minded" people who do not trouble themselves by speculating about dreams than that the soul has not only an existence apart from the body in the next world, but that it also leaves the body during its sleep to make excursions in this world?

The fundamental fact of science is, that what the brain is at any moment, so the convictions (which are, indeed, nothing else than beliefs) will be. The scholastic archbishop, knowing this practically, if not scientifically, as a matter of course earnestly and persistently aims at the possession of the youthful brain when in course of development, so that he may be able to mould it suitably for the beliefs essential to constitute a good Catholic in after life. Those skilled educators the Jesuits would entirely agree with Dr. Carpenter in the subjoined statement as to the influence of the will on belief:

"Since preformed mental habits determine, not merely the primary acceptance or rejection of a Proposition, but the issue of further attempts to *make it fit*—not only will different individuals draw very different conclusions from the same data, but the same Ego will form different judgments as to the very same matter at different stages of mental life; without any change in the external materials of his judgment, but solely from changes in his own fabric of thought." (p. 396).

Whether the fabric here referred to be the brain as it is evolved by age and experience, or whether it is a merely metaphorical expression, does not appear; but unquestionably the Roman Archbishop of Westminster, the most outspoken advocate of the Syllabus, knows well the danger of different judgments being formed if a man be left free to exercise his brains and speculate about dogmas. Hence the Roman Catholic is expected to surrender his right of judgment to "the Church," meaning the Pope and clergy; hence the Church guards, not only his training, but his subsequent reading, bids and forbids what books he shall read and what studies he shall follow, and adopts every available means to compel obedience, so that there shall be no "self-determining power" of judgment, but automatic Catholic activity of mind alone.

It is almost idle trifling to show that a man's beliefs and convictions only represent brain-work, and must therefore correspond to that, whatever it may be. But it is most impor-

tant to show that ignorance of this fundamental fact of mental science is at the root of errors in religion and morals of the most dangerous character. Archbishop Manning is himself a most striking example, and a warning to every man who would keep his brain clear of delusions. A hysterical nun, trained up from childhood in ecclesiastical discipline of a kind the most dangerous to soundness of brain, reported to her confessor no fewer than seventy apparitions of Jesus Christ to her. These accounts of mere hallucinations and insane delusions, if true, are of the most revolting kind, as they have been detailed by two authorised clerical historians, namely, Canon Cucherat, chaplain at Paray-le-Monial, and Leon Aubineau. With these writers Archbishop Manning believes that these hallucinations and delusions are facts. They believe that the wounds inflicted upon herself by Marie Alacoque afforded proof that her heart had been removed operatively from her thorax by Christ and returned after being partly consumed by His heart. The metaphorical expression that the heart of Jesus Christ burnt with the love of mankind is taken literally; they believe anatomically in a real heart, psychologically the seat of emotion, and in real flames, and with the Pope, whom he holds to be infallible in his convictions, Archbishop Manning is a worshipper of "The Sacred Heart," and a high priest of the new cultus, the Jerusalem of which is at Paray-le-Monial. To this place he led a troop of believing Catholics in solemn pilgrimage, with as much foundation in physical fact as a pilgrimage to Mecca or Juggernauth.

There is nothing, however, in these views of Dr. Manning that is either new or true. The belief in hallucinations and delusions, and the artificial excitation of them as "pious frauds," have prevailed from time immemorial all over the whole world, amongst all races. If Dr. Manning will refer to Mr. Tyler's interesting work he will find ample proof of this.[1]

How deadly and how destructive to true religion are the superstitions and delusions that have been thus fostered cannot be here stated. It has ever been the duty of the medical profession to enlighten mankind as to the true character of these delusive convictions, and certainly with a measure of success. Even now, however, to exorcise devils from the body of a maniac is an orthodox procedure with the Roman Catholic Church, and to represent souls as suffering the torments of physical fire a sacred duty. With the medical profession the true orthodoxy is an unquestioning allegiance—not to convictions and opinions held as beliefs—but to the duty of inquiring

[1] See 'Primitive Culture,' vol. ii, *passim.*

whether convictions and opinions correspond to the order of nature.

We may here observe (to be quite just) that an equally instructive warning as the lamentable fall of Archbishop Manning is given by the subjugation of certain scientific minds to the dogmas of spiritualism. Mr. Cox being only a lawyer, may be excused the erroneous conclusions to which he has been led by his error in method. Not so scientific men like Mr. Wallace and Mr. Crooks, who have rivalled the archbishop in their wanderings from truth and fact as to the order of nature. Mr. Wallace has published an account of modern spiritualism in the 'Fortnightly Review,'[1] in which the methods followed and the conclusions deduced are fully stated. We subjoin an example:

"*Spiritual forms.*—These are either luminous appearances, sparks, stars, globes of light, luminous clouds, &c.; or hands, faces, or entire human figures, generally covered with flowing drapery except a portion of the face and hands. The human forms are often capable of moving solid objects, and are both visible and tangible to all present. In other cases they are only visible to seers, but when this is the case it sometimes happens that the seer describes the figure as lifting a flower or a pen, and others present see the flower or the pen apparently move by itself. In some cases they speak distinctly, in others the voice is heard by all, the form only seen by the medium. The flowing robes of these forms have in some cases been examined and pieces cut off, which have in a short time melted away. Flowers are also brought, some of which fade away and vanish; others are real, and can be kept indefinitely. It must not be concluded that any of these forms are actual spirits; they are probably only temporary forms produced by spirits for purposes of test or of recognition by their friends. This is the account invariably given of them by communications obtained in various ways, so that the objection once thought to be so crushing—that there can be no ghosts of clothes, armour, or walking sticks—ceases to have any weight." ('Fortnightly Rev.,' June, 1874, p. 793.)

The series of groundless assumptions contained in this extract is a sufficient example of the method followed. It is assumed as a fact of science that there are spirits by whom all these phenomena of sense are excited. They produce at will luminous appearances of various kinds, hands, faces, vocal apparatus, entire bodies, flowers, clothing, armour, cocked hats, and pigtails, anything, in short, which can be presented to the morbid states of consciousness as spectral illusions or hallucinationts or delusions. The robes cut off melt away; but why did

[1] May and June, 1874.

not Mr. Crooks examine first into their chemical composition? So do the flowers sometimes, but others are real; why did not Mr. Wallace inquire how and whence they came? A spirit produced to the assembled inquirers a sunflower six feet high, with earth still adhering to its roots. Was this a phantom? If not, did the spirit create the earth about the roots as well as the flower?—or did he grow the plant? or buy it? or steal it? A dress lately examined was found to be calico with plenty of lime and paste to stiffen it. Surely Mr. Wallace and Mr. Crooks could have learnt whence the calico and sunflower came.

But it is held that embodied as well as disembodied spirits can create and produce in this way. In one instance a lady in a trance produced her double, which was found to be as solid a corpus, on being taken to Mr. Crooks's arms, as the lady herself; yet the simplest means were not taken to discover whether it was not the lady herself in disguise.

It is not surprising after this that Mr. Wallace finds historical proofs of his conclusions in the various delusions as to a "spirit world" which have infected mankind from time immemorial, such as Socrates' demon, the Pythian priestess, demons and demoniacal possession, second sight. If the bloody animism and spirit superstitions of Ashanti and Dahomey are not included in the historical catalogue it is not because they are not less credible than the others. But what is perhaps the most painful element in this exposition of spiritualism is its orthodox and theological element. Spiritualists profess to explain how the water was changed into wine by Christ, and how he fed 5000 persons miraculously. The abundant miracles of Rome, too, are, in like manner, rendered intelligible facts by the conclusions of Mr. Wallace. Such, for example, as the apparitions of the Virgin at La Salette and elsewhere are merely the artificial production of spirits enthusiastically anxious to excite religious fervour, being sham appearances of the Virgin—pious frauds, in short—done for the express purpose of inducing a false belief. So also as to the miracles and miraculous appearances of deceased saints. It is only an erroneous inference of the deluded Catholic that the real virgin or saint appears; "an inference," Mr. Wallace remarks with a touch of scientific cant, "which every intelligent spiritualist would repudiate as in the highest degree improbable."

That scientific men should thus become the slaves of dominant mystic ideas is the strongest proof that the most highly cultured have no "self-determining power" apart from cerebral organization. As Plato said of the depraved, no man is voluntarily wicked, so these are not willingly deluded.

A knowledge of that organization which is the means whereby

we acquire all knowledge, and exercise all our faculties without exception, is essential to preserve the wisest from error. Dr. Carpenter closes his first chapter by affirming "that the Psychologist may fearlessly throw himself into the deepest waters of speculative inquiry in regard to the relation between his mind and its bodily instrument provided that he trusts to the inherent buoyancy of that great fact of consciousness, that *we have within us* a self-determining power which we call Will." In this passage, apart from a knowledge of cerebral physiology and pathology, we have a "bootless boast." The "inherent buoyancy" only carries the inquirer devoid of scientific ballast into the realms of mischievous delusions and hallucinations.

And such seems to be the fate of the learned serjeant-at-law Mr. Cox, who, practically ignorant of cerebral anatomy, physiology, and pathology, has investigated the phenomena of "spiritualism" according to the legal method of inquiry, and has come to the conclusion that he has discovered a new force in nature, the phenomena of which constitute those of Life and Mind. The whole book is an illustration of a common form of mysticism—the result of evolutional brain-work as to force and energy, and very common as a morbid manifestation in the insane.

Briefly, the chief alleged facts as to this new force are as follows:

"1. That there is a force other than the Forces of Nature hitherto recognised. But whether it is the one Force, which is said to change its form according to the substance in which it is exhibited, or a Force entirely distinct from the human Physical Forces, and subject to other laws associated with vitality, there is not as yet sufficient evidence to determine. 2. That this Force produces positive sounds and motions in [and of] solid bodies [articles of furniture] brought under the radius of its influence. 3. That this Force is found to operate at an undefined but not an indefinite distance from the human body. 4. That it is developed (so as to be perceptible to the senses by its effects) in certain persons only, to whom the name of Psychics has been given. * * * 8. That there is some but not sufficient evidence that Psychic Force, and what physiologists have termed 'vital force,' and Dr. Richardson the 'nerve-ether,' are identical. * * * 11. That the Psychic Force is controlled and directed by the intelligence of the Psychic. That this intelligence frequently acts without consciousness of the Psychic. But if such action is that of the brain, or of an individuality distinct from the brain and incorporeal, there is no sufficient evidence. 12. That the condition of the Psychic during such unconscious direction of the Force is generally similar to, if not identical with, that of the somnambulist, whose intelligent acts are the result of unconscious action of the brain, which not only dreams, but causes the patients to act the dream." (vol. ii, p. 287-9.)

These conclusions and others are drawn from observations made by the learned serjeant as a member of a committee of the "London Dialectical Society," appointed to investigate into the pretensions of Spiritualism. Of these investigations Mr. Cox remarks:

"Here is evidence which, in any court of justice in the world, would be held to be conclusive proof of *the fact* asserted by the witnesses whose honesty and capacity nobody has questioned. If so palpable a fact as the motion of an untouched table cannot be received on the testimony of so many observers specially charged with the duty of noting and testing, truth in any matter must be unattainable, and treatises on evidence a mockery. All the facts of Science must be equally denied, for not one of them is established by better evidence than this fact of motion without contact," &c.

"*All* the facts?" May we not without presumption suggest to the learned serjeant that motion by contact, the fundamental fact of the science of motion, is now universally admitted to be a fact? And perhaps motion by contact, when a man has knocked another man down by hitting him on the head with a chair or table, or even throwing it at him, would be more easily proved in any court of justice than that the delinquent knocked his enemy down by said table or chair without touching it—impelling it, in fact, by his evil will or eye.

In his first volume Mr. Cox undertakes to give a popular exposition of "the mechanism of Mind;" in his second he explains "the mechanism in action." What exact knowledge he has of so complex and wonderful a structure as the human brain may be gathered from the subjoined exposition of its anatomy:

"The frontal section of the brain, technically termed the *cerebrum*, fills the frontal cavity of the skull. Then comes the middle lobe. Behind it and annexed to it is a third lobe, called the *cerebellum*, whose structure is more pulpy and less fibrous than that of the *cerebrum*.

"At the base of the brain is a pulpy mass, the *medulla oblongata*, lying between the brain and the spinal cord, and from this there extends another ganglionic mass, occupying the whole base of the brain, running almost from the back to the front of the hemispheres, and thus being in direct connexion with every part of it. * * * To this centre have been traced the extremities of the nerves of the senses," &c. (vol. ii, p. 149.)

Where Mr. Cox got his knowledge of this cerebral anatomy does not appear; it may be certainly said not from dissection, and it may almost be as safely said that it is a creation of his imagination out of his reading of "Carpenter and Marshall," whom he names together. Now, if derived from the former, he has confounded Dr. Carpenter's *cerebrum*, which means simply

the convolutions of the hemispheres (the hemispherical ganglia of Solly), with the *cerebrum* of the anatomists, which includes all the encephalon except the cerebellum, medulla oblongata, and pons. The "ganglionic mass occupying the whole base of the brain" of the serjeant is evidently an imaginary representation of Dr. Carpenter's "sensorium" or "aggregate of sense ganglia." Further, Dr. Carpenter has also a drawing of the nervous system of man in the form of a naked human figure, coloured black, with limbs in suitable attitudes, on which are represented diagramatically (not anatomically) the trunks and branches of the nerves. It is a small popular cut, but it seems to have wakened up the learned serjeant's creative imagination most effectively. He has, he affirms, a strong conviction—

"That the body is shaped in precise accordance with the shape of the nerve-system and the lines of force flowing from it, and that a man is really a nerve system clothed in flesh, and not, as we are accustomed to think of him, a structure of bones and flesh permeated by nerves and shaped by some external formative force whose source and action are equally unknown to us." (vol. i, p. 25.)

Again, by a cunning trick of this imaginative energy Mr. Cox converts the doctrine of "unconscions cerebration," originally promulgated to explain table-turning, to his own uses, and quotes it in proof of his psychic force. But most curious of all as a psychological instance is the fact that Mr. Cox turns all that he reads to like uses, or else, when he cannot transmute the facts, denies them as contrary to evidence. Assuming that, being a lawyer, he is a competent judge of scientific methods, he observes:

"In the course of the composition of this little book my attention has been often and forcibly directed to the extremely unsatisfactory character of scientific evidence. To a lawyer, who has imbibed from his youth up the Principles of Evidence, and who has been trained, by experience in courts of justice, to the almost instinctive recognition of what does or does not constitute *proof*, there is nothing so surprising, and at the same time so vexatious, as the almost entire disregard of the plainest principles of Evidence by the votaries of Science. * * * This disregard of the most elementary rules of evidence is especially remarkable in works that treat of Physiology, Medicine, and Psychology," &c. (Preface, p. 12.)

We can judge of the serjeant's "principles" by their outcome in conclusions.

If Mr. Cox had lived in the time of Matthew Hale, like that learned judge he would have been a believer in witchcraft. Happily for him and society medicine has expelled that class of delusions from our jurisprudence, but Mr. Cox renders it clear that the forensic method of inquiry which led Chief Justice

Hale to condemn a woman for witchcraft still flourishes. The late Lord Westbury, when Lord Chancellor, expressed exactly the same legal opinions as to "facts" which we find in Mr. Cox's volume. Speaking of insanity he said, "The introduction of medical opinions and medical theories into this subject has proceeded upon the vicious principle of considering insanity as a disease, whereas the law regards it as a fact which can be ascertained by the evidence, in like manner as any other fact." In truth, the learned serjeant rides his forensic notion of what are "facts" to the death. The book is valuable as an instructive illustration of the chief cause of the law's uncertainties and delays.

The work of Mr. Woodward is one of a class very familiar to the philosophical reader. A thinker, he deals with words rather than facts, and evolves out of his thinking a mystic hypothesis of the "triune" nature of man, the three elements being Love, Reason, Man, and these words are defined in a poetical summary to be found in 'The Hermit,' a poem by the author:

> "God is love—author of all things;
> Spirit is reason—man's intelligence;
> Body is Christ—the perfectness of man."

We do not attempt to review the book, for we cannot understand it. We gather, however, that the fundamental ideas came to the author by a sudden inspiration when meditating philosophically. We learn, too, that his "conception of the triune nature of Man exactly correlates the *substance* of the Athanasian Creed." But although of the reflective school, Mr. Woodward, like Mr. Cox, sees the importance of sound physiological knowledge as a basis of "practical philosophy" though he has it not. And upon the whole, although we are not able to understand his views, we respect him for the tone of his work and the good objects of his philosophy.

IV.—Klein's Researches on the Lymphatic System.[1]

THIS work constitutes an extremely valuable contribution to our knowledge of the lymphatic system. It results from a series of observations undertaken by the author originally in conjunction with Dr. Burdon-Sanderson, but subsequently continued alone, and it is published with the sanction and approval

[1] 1. *The Anatomy of the Lymphatic System.* By E. KLEIN, M.D., Assistant Professor at the Laboratory of the Brown Institution. I. The Serous Membranes. London, 1873.
2. *Contributions to the Normal and Pathological Anatomy of the Lymphatic System of the Lungs.* By E. KLEIN, M.D., &c.
3. *The Lymphatic System of the Cornea.* By E. KLEIN, M.D., &c.

of Mr. Simon. The present division of the subject is, it will be seen, limited to the serous membranes, and Dr. Klein treats separately—1, of the endothelium of the free surface; 2, of the cellular elements of the matrix; 3, of the distribution and relations of the lymphatics; and 4, of the blood-vessels of the serous membranes; and we shall endeavour to condense in the following pages the remarks made on each of these points.

In the first place in regard to the endothelium. In most of our text-books it is merely stated that the epithelium lining the interior of the serous membranes is of the tesselate variety, but Dr. Klein shows that in the normal condition it is not everywhere a layer of flattened, more or less hyaline cell-plates, but that it possesses in many places a different character, the individual cells being polyhedral, club-shaped, or even low columnar in form, with distinctly granular substance even in the fresh condition, and with ovoid or sometimes spherical, clear, sharply defined nuclei containing a large shining nucleolus. A portion of the fenestrated part of the fresh *omentum* of a full-grown guinea-pig, cat, dog, or monkey, mounted in an indifferent fluid, such as a one half per cent. solution of common salt, peritoneal fluid, &c., or after having been stained in one quarter or one half per cent. solution of nitrate of silver, and mounted in glycerine, frequently exhibits on the surface of the thin trabeculæ small groups of club-shaped or polyhedral granular cells projecting from the surface, especially of the larger trabeculæ-like buds. These cells Dr. Klein terms *germinating* or *young* endothelium; they are raised from the general surface by means of a stalk and possess in their peripheral portion two nuclei or a nucleus in a state of division; and, further, spherical cells exactly resembling lymphoid cells are seen to be formed here, which are either simply attached to the general surface or in the act of separating from young endothelial cells. The same character is possessed by the endothelium of the surface of certain *nodular* or cord-like structures, which are either isolated or stand in connection with the chief trabeculæ of the fenestrated part of the omentum, in which large blood-vessels or fat are contained. In addition to the omentum the *pleura mediastini* of dogs, cats, and monkeys, is a very good subject for studying germinating endothelium. The trabeculæ present nodular swellings, on the surface of which is abundance of the young endothelium, and here also, from the great number of club-shaped and pedunculated, bi-nucleated endothelial cells, and from the number of spherical cells which are more or less loosely attached to the surface of the above-mentioned swellings, it seems to be certain that a very abundant production of lymphoid cells is here taking place.

Similar germinating endothelium is to be found during the winter months over the trabeculæ of the *mesogastrium* of the female frog, the cells here possessing the peculiarity of being ciliated. Mr. Francis Darwin showed Dr. Klein a preparation of the mesogastrium of the *Rana temporaria* in which distinct amœboid movements were visible in the germinating endothelium; an analagous but not quite so abundant germination may be observed on the peritoneal surface of the *septum cisternæ lymphaticæ magnæ* of the frog, especially on the endothelial cells which surround or, more correctly speaking, line the stomata.

The germinating endothelium of the *mesentery* of frogs, cats, dogs, and monkeys, presents a somewhat different arrangement, for in these membranes the cells only occur in groups of less than five and for the most part of only two or three.

Besides the above-described germinating endothelium Dr. Klein states that he has found branched cells, the body of which is situated at the point of junction of a number of endothelial cells, and the processes of which stretch between the endothelial cells, so as to become identified with the sinuous contour lines of the latter brought into view by means of nitrate of silver; some of these branched cells contain a well-defined clear nucleus. In some preparations slightly stained with silver he has been able to make out with high powers that the body of these branched corpuscles lies rather deeper than the endothelium, whereas one or the other of the finely granular processes stretches itself quite superficially between the endothelial cells.

2. In regard to the cellular elements of the matrix or ground substance, Dr. Klein considers the omentum of rabbits to be best adapted for observation, as being flatter and for the most part very slightly fenestrated compared with that of the guinea-pig, dog, cat, and monkey. The rabbit should be killed by bleeding and the stomach exposed; the intestine is then pushed to the right side, and the free surface of the omentum is pencilled several times from the large curvature towards the diaphragm with a fine camel's hair pencil moistened with fluid of the abdominal cavity. After this a quarter or one half per cent. solution of nitrate of silver is allowed to flow over the omentum from a large capillary tube until the membrane has become slightly milky (one to two minutes are generally sufficient); the stomach, together with the omentum, spleen, pancreas, and a portion of the duodenum, are then cut out and transferred to a large capsule with distilled water; after some time the water is renewed and the omentum is separated under water, together with the spleen and pancreas, from the stomach, with scissors,

and is transferred to common water. Those portions of omentum which are seen to contain small patches are cut out and mounted. A failure, he adds, is more frequent than a success. Either the pencilling has been done too little or too much. Successful preparations become only slightly yellowish-brown, and can be preserved without alteration for a very long time. On microscopic examination of the matrix brownish patches composed of nucleated cells are seen, whilst the matrix itself is perfectly unstained. The brownish cells vary in size, but are always much larger than colourless blood-cells; their body is beset with a variable number of smaller or larger blunt or roundish prominences. Some of the cells are sharply defined, others are on one side more or less distinctly continuous with the basis on which they lie. It is clear they correspond, as regards shape, to migratory cells. Besides these cells there are to be found a limited number of small granular corpuscles with one or two small nuclei, which exactly resemble lymphoid corpuscles. The matrix proper unstained by the above procedure presents a network of a finely granular substance, which is stained slightly yellowish. This network consists of large plates connected with each other by shorter or longer tracts of different breadth. In the plate-like enlargements, of which he gives an excellent drawing, a sharply defined oblong nucleus with one or two nucleoli may be recognised, or a nucleus which has almost divided into two. It is evident from this figure that we have before us a network of branched, more or less flattened cells, which on the one hand undergo division, and on the other hand give origin to large, coarsely granular elements. If our attention be directed to a small patch which is provided with a system of capillary blood-vessels, we see that nearly the whole matrix is occupied by a network of finely granular, nucleated, branched cells, which network is in communication with the isolated branched cells of the surrounding tissue. In other parts lacunæ communicating by canaliculi, and containing branched cells and lymphoid corpuscles, may be seen, which constitute the well-known lymph-canalicular system of Recklinghausen.

In regard, therefore, to the cellular elements of the matrix of the omentum, Dr. Klein takes the same view of them as Rollett does of those of the cornea. In both cases the branched cells are more or less flattened parallel to the surface, and in both cases the lymph-canalicular system corresponds to those cells. In the omentum as well as in the cornea the migratory cells are always found in the lymph-canalicular system. There cannot, he thinks, be the slightest doubt about the cellular elements of the matrix being more or less flattened, branched,

nucleated protoplasmic cells, whilst the lymph-canalicular system represents the spaces in the ground substance for those cells. He sums up his account of the patches and tracts above described, which he terms lymphangial patches (nodules) and lymphangial tracts, in the following terms:

"In the omentum of the rabbit there exist two kinds of lymphangial structures—(*a*) Patches the matrix of which consists of groups of ordinary more or less flattened, more or less branched cells, which on the one hand multiply by division, in which way the patch increases in size, and from which on the other hand grow up lymphoid cells. At an early stage of development these patches do not contain a special system of blood-vessels; at a later time they possess a special rich system of mostly capillary blood-vessels. By growing in length these patches join so as to form whole tracts.

"(*b*) Patches and tracts the matrix of which consists of a reticulum, the meshes of which contain a variable number of lymphoid corpuscles; they are generally provided with more or less abundant blood-vessels."

The omentum of the guinea-pig, cat, dog, and monkey, has a very similar structure.

Dr. Klein then proceeds to describe the development of adipose tissue in the gelatinous body of the infra-orbital fossa, and by a comparison with this shows that the more or less isolated fat-lobules of the mesentery and other serous membranes are only transformed perilymphangial nodules. As long as a lymphangial nodule does not become converted into a fat-nodule there are always numerous lymphoid cells to be found in it, which are partly, at least, to be considered as offshoots of the branched cells of the stroma of the nodule. But as soon as a lymphangial nodule becomes converted into a fat-nodule the lymphoid cells become fewer. When the lymphangial nodules and tracts are in process of becoming converted into fat-tracts they grow relatively quickly. Dr. Klein is in accord with Fleming in saying that fat-cells are transformed branched cells, but he differs from him in saying that fat-tissue develops in the adventitia of arteries.

3. In regard to the lymphatic vessels of the serous membranes Dr. Klein remarks that in the rabbit's omentum they are easily demonstrated. They are wide vessels, the walls of which consist of only one layer of endothelial plates; they are related to the large blood-vessels in such a way that a group of the latter has a lymphatic running along both sides, the two communicating with each other by transverse and oblique lateral branches. These have valves and sacculate dilatations corresponding to them. There are also smaller capillary vessels, which are desti-

tute of valves. Not unfrequently blood-vessels run within the lymphatics, and between the walls of the two vessels are lymphoid cells. In following out such investing lymphatics they are seen to become continuous with or to break up into a labyrinth of spaces, which consist of lacunæ with uniting canals, representing the lymph-canalicular system. Instead of a single vessel, it may sometimes be seen that a wide lymphatic vessel invaginates a whole system of capillaries. From a general review of the relations of the capillaries and the lymph-canals, lymph-canalicular spaces, and cells in continuation with them, Dr. Klein differentiates two forms of nodules, *peri-* and *endo-lymphangial nodules*, the former constituting isolated nodules or patches and, by fusion, cords, the latter forming also patches and nodules and, by continuous outgrowth, cords. The development of the perilymphangial nodules follows more the arterial vessels, whereas that of the endolymphangial nodules the venous vessels. In the *perilymphangial patches* the lymphatic vessels lose themselves in a labyrinth of spaces which consist of lacunæ with uniting canals, representing the lymph-canalicular system, and, on the other hand, the endothelial plates of the lymphatics are continued as branched cell-plates corresponding to what Recklinghausen first described as the origin of the lymphatic capillaries in the centrum tendineum of the rabbit, with the addition of the fact that the endothelium continues itself in the lymph-canalicular system in the form of branched cell-plates.

The *endolymphangial nodules* proceed from the growth of a network of branched cells developed from the endothelium of the lymphatic sac, whereby the lymphatic vessel is transformed into a cavernous or sinuous structure. The ultimate result is a delicate reticulum, with lymphoid cells in its meshes, and supplied with a more or less rich capillary network.

Dr. Klein describes fully the lymphatic system of the centrum tendineum of the diaphragm, and his researches confirm and extend those of Recklinghausen, Ludwig, and Schweigger-Seidel, all of whom have been laborious workers in this field. He gives us a good method of injecting the lymphatics by the following plan. Into the abdominal cavity of a living, middle-sized, fasting guinea-pig, he injects a few centimètres of a five per cent. solution of Brücke's Prussian blue. After from four to eight hours the animal is killed by bleeding from the crural artery, and is allowed to become cold. In the rabbit the lymphatics of the centrum tendineum are arranged in two systems for each half, an anterior and a posterior. The vessels of the anterior system are distributed upon the outer and anterior (larger) portion of the anterior quadrant, and upon the outer (smaller) portion of

the posterior quadrant. The posterior system is distributed in the corresponding and opposite quadrants. Each system communicates with the other by a few large vessels. The efferent trunk of the anterior system converges to one or two large vessels, which run with the mammary vessels towards the sternal gland. The efferent trunk of the posterior system is single, and opens into the thoracic duct just above the diaphragm. There are two kinds of capillaries—those that chiefly lie in the pleural serosa and those that run in a straight direction parallel to and between the fasciculi of the tendinous tissue. The straight vessels communicate with the peritoneal cavity by means of stomata, and Dr. Klein maintains, developing and extending Ludwig's views, that the lymphatics are widely dilated during the inspiratory action of the diaphragm, whereas they are compressed during expiration, the lymphatics of the pleural surface of the diaphragm being exactly the reverse. "This, however," he goes on to say, "is not the only way in which the straight lymphatics act. We have mentioned before that the lymphatics of the diaphragm are arranged in an anterior and posterior system, the former discharging itself in trunks that run towards a gland, the latter in a wide short trunk that runs directly into the thoracic duct. Now, the straight lymphatic capillaries are the vessels which effect the communication between these two systems, and this is their chief action. As the deep straight lymphatic capillaries and the superficial ones represent, as we have seen, only one category of vessels, we are justified in saying this category of lymphatic capillaries discharges itself in two directions, one freely in the thoracic duct, and a second less freely towards the sternal gland. In a following chapter we shall see that the straight lymphatic capillaries, the deep as well as the superficial, stand in free communication with the peritoneal cavity by means of vertical lymphatic canals (stomata of authors). Consequently we have to substitute for the diagram of Ludwig and Schweigger-Seidel—representing the lymphatics as a single pump—a diagram of a pump with two cylinders, the one cylinder corresponding to the pleural vessels of the anterior system, the other to those of the posterior system, while the pipe connecting the two cylinders is represented by the straight capillaries, and the piston pipe by the vertical lymphatic canals. It must be borne in mind, however, that the two cylinders act simultaneously." He then describes the lymphatic vessels of the mesentery, and goes on to show the relation of the lymphatic vessels to the surface of the serous membranes, especially in regard to the stomata, of which he distinguishes two varieties, the stomata vera and the stomata spuria, and gives the distinguishing cha-

racter of each ; and lastly, in regard to the blood-vessels of the serous membranes, he passes briefly over their distribution as being generally well known, but dwells at some length upon their mode of development, which he shows to be from the vacuolation and junction of branched cells.

The account of the pathological changes which take place in the serous membranes are full of interest, but would occupy too much space to be here given in full. The book is illustrated by a series of ten beautifully executed plates, containing fifty-four separate figures, and greatly aiding the reader in the comprehension of the text. Altogether the work is a very creditable outcome of researches undertaken in the Brown Institute, and we hope is the prelude to many more.

The second paper placed at the heading of this article was published in the 'Proceedings of the Royal Society' (No. 149, 1874), and is a continuation of the larger treatise just noticed. In it Klein shows that the endothelium of the surface of the lungs consists, in the normal condition, of polyhedral cells (not flattened, as usually described) arranged in a single layer. The endothelium of the pleura costalis is flattened and hyaline.

The pleura pulmonum is a very thin connective-tissue membrane, provided with a rich network of elastic fibres. In the guinea-pig there is a layer of non-striated muscular fibres, arranged in bundles, which form a network, and most abundant over those parts of the pulmonary surface which are subject to excursions of the greatest extent. In rats and rabbits, as well as in cats and dogs, bundles of unstriped muscular fibres occur sparingly. He describes a system of subpleural lymphatics, a system of perivascular lymphatics, and a system of peribronchial lymphatics. His account of the origin and distribution of these is essentially similar to that above given.

In the third short pamphlet, originally published in the 'Lancet,' Dr. Klein states that he has frequently observed in sections of the cornea, treated in the usual way by nitrate of silver and chloride of gold, singly and combined, appearances that could only be explained by supposing the existence ef epithelium-lined lymphatic vessels running through the structure. Recently, in re-examining preparations that had been laid aside for a time, he found that in some of them these appearances had developed to an extent and precision that place the existence of lymphatic vessels in the cornea beyond doubt. Several drawings are given.

V.—Pseudo-Muscular Hypertrophy and its Relations to Muscular Atrophy.[1]

THE affection of the muscular system which we here propose to consider is one that had, until comparatively lately, escaped attention. It may, therefore, be a not unprofitable study to review our knowledge of its pathological characters, its relation to cognate affections, and its therapeutics. The cases hitherto recorded, although not sufficient to permit us to dogmatise with confidence on all or either of the above points in its history, may, nevertheless, suffice to direct our future efforts in the investigation of its real nature and its close relationship to progressive muscular atrophy. The views entertained as to the pathology of the disease have in this, as in many other instances, been embodied in the designations conferred upon it. And, as in many other like instances, the designations have been altered as theories have changed. By Duchenne it was at first named "paraplegie hypertrophique de l'enfance de causes cérébrales;" subsequently, eliminating one hypothetical element, the same author named the malady "paralysie musculaire hypertrophique," or "paralysie myosclerosique." M. Jaccoud has employed the term "sclérose musculaire progressive," a sentence open to the objection that it may apply with equal correctness to progressive muscular atrophy. Other designations have been proposed, *e.g.* "Duchenne's paralysis," "paralysie myosclerose," indicative of personal recognition, or suggestive of the predominance of a neurotic, or a lipomatose theory in the minds of the proposers. The title "pseudo-muscular hypertrophy" is, on the whole, least open to objection, since it conveys at once the idea of its leading symptom, and leaves its pathology an open question.

To this question of its pathology we, in the first place, direct attention. Its alliance with the ordinary forms of muscular

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[1] 1. *De la Paralysie Musculaire Pseudo-hypertrophique.* Par M. DUCHENNE, de Boulogne. Paris, 1868.

2. *Note sur l'état des Muscles et de la Moelle epinière dans un cas de Paralysie Pseudo-hypertrophique.* Par M. CHARCOT. 'Archives de Physiologie.' Mars, 1872.

3. *Beiträge zur Kenntniss der Atrophia Musculorum lipomatosa.* Von Dr. OTTO BARTH. 'Archiv der Heilkunde.' Leipzig, 1871.

4. *Ueber der Progressive Muskelatrophie, ueber wahre und falsche Muskelatrophie.* Von Dr. N. FRIEDREICH. Berlin, 1873.

5. *Cases of Pseudo-muscular Hypertrophy.* By W. B. KESTEVEN. 'Journal of Mental Science,' 1870-71.

6. *Condition of the Muscle in Pseudo-hypertrophic Muscular Paralysis.* By HENRY T. BUTLIN. 'St. Bartholomew's Hosp. Reports,' 1872.

7. *A Case of Pseudo-hypertrophic Muscular Paralysis.* By J. LOCKHART CLARKE, M.D., and W. R. GOWERS, M.D. 'Medico-Chir. Trans.,' 1874.

8. *A Case of Duchenne's Pseudo-hypertrophic Paralysis.* By WILLIAM M. ORD, M.B. 'Medico-Chir. Trans.,' 1874.

atrophy is, although it may seem to involve a contradiction, the one prominent feature brought out by its pathological investigation. Dr. Friedreich, whose work contains the most extensive series of cases of both forms of muscular affection, arrives at the conclusion that pseudo-muscular hypertrophy is essentially the same lesion as progressive muscular atrophy, differing only in certain immaterial modifications. The anatomical basis of the malady consist in an excess of areolar and connective-tissue spreading amongst and apparently increasing the bulk of what are really degenerated muscular fibres. In advanced cases, the fibres have been wholly replaced by fat and areolar tissue. The details of these changes are fully described and accurately delineated in the treatise of M. Duchenne, by Mr. H. T. Butlin,[1] Dr. Clarke, and M. Charcot.

Dr. Friedreich regards the morbid condition of the muscular fibres as one of chronic inflammation, a true myositis, concurrent with hyperplasia of connective tissue, whence are generated the elements of fat, forming a diffused lipoma. He does not concur with Duchenne in considering it an "espèce morbide nouvelle," but as essentially the same as progressive muscular atrophy, to which we shall refer subsequently. We have, he adds, in both a wasting of the primitive fibres, sometimes with disappearance of the transverse striæ and the appearance of fatty degeneration; in both there are found hypertrophied muscular elements intermingled with the atrophied fibres; in both we have evidences of the inflammatory process in the proliferation of nuclei and corpuscles; in both there is the presence of hyperplasia of connective, as seen in cirrhosis of the liver and other organs; the only difference recognised is the excessive development of fat in pseudo-hypertrophy. Even as far as regards this distinction Dr. Friedreich observes that the line of demarcation is not so distinct but that the characters of the two affections are sometimes fused. The view thus pronounced is certainly supported by the clinical and etiological history of both forms of disease.

Passing from the consideration of the pathological conditions found in the muscles, we come to those observed in the nervous centres. These have not been so largely studied as in cases of muscular atrophy, the opportunities having been fewer from the fact that the affection itself has not always been recognised. The results at present obtained ar ccording to Dr. Friedreich's survey, rather of a neg han positive character. The majority of observers report integrity of the nerves and nerve-centres. This statemen , however, be accepted

[1] 'St. Bartholomew's Hospital Reports,' 1872.

with a deduction for the absence in many instances of scientific histological investigation. Amongst the most important contributions of competent and trustworthy microscopical observers are those by Dr. Barth, Dr. Muller,[1] Mr. Kesteven, and more recently by Dr. Lockhart Clarke. Dr. Barth describes the principal lesions as consisting in gelatinous degeneration of the neuroglia in the anterior grey matter and lateral columns, with corpora amylacea, and atrophy of nerve-cells. Müller reports the presence in excess of fat-cells in the cellular tissue around the spinal cord, and depicts certain fatty deposits in the substance of the cord. These fatty deposits bear an exact resemblance to the spots of miliary sclerosis that have been described by Dr. Batty Tuke in the pages of this journal. In a recent communication the relation of cause and effect was attributed by Mr. Kesteven to miliary sclerosis in the case described, but it appears that he has since seen reason to give to this lesion a much wider pathological significance. It remains, therefore, doubtful how far the alterations in the brain or spinal cord observed in these cases are to be regarded as primary and causative, or consecutive and consequential. From a careful consideration of the opinions and cases hitherto published, the balance seems to incline to the opinion that pseudo-muscular hypertrophy is a disease primarily seated in the muscles, which are in a uniformly morbid condition, while the lesions of the nervous centres, which are by no means uniform, are secondary, or at least coincident only, and very far from serving as a means of distinction between this disease and progressive muscular atrophy. Dr. Lockhart Clarke, however, and Dr. Bastian have explicitly expressed the opinion that this affection depends upon changes in the structure of the spinal cord. Dr. Clarke had described extensive changes in the grey matter of the cord, *e. g.* degeneration and atrophy of nerve-cells. In a recent report upon another case Dr. Clarke has also recorded the occurrence of very extensive lesions of the grey matter in every region of the cord, implicating also the anterior commissure and the posterior roots in the masses of disintegration. It has, however, been forcibly observed by one of our contemporaries that the anatomico-pathological facts related serve, no doubt, to account for the paralysis, but do not explain the changes in the muscles.[2]

Dr. Friedreich very [illegible]phatically maintains the argument that pseudo-hypertroph[illegible]iginally and essentially the same as progressive atrophy, [illegible] former the disease being modified

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[1] 'Beiträge zur pathol. [illegible]at. des Mensch. Ruckenmarks,' Leipzig, 1871.

[2] 'Ed. Med. Times and Gazette,' June 6, 1874.

by age and a special tendency to the deposition of fat and areolar tissue. Any changes, he observes, that may be found in the nervous centres are to be regarded as secondary, and any apparent intellectual deficiency to be attributed to general backwardness and defective education.

M. Charcot also combats the opinion of the spinal origin of pseudo-muscular hypertrophy. From the microscopical examination of a series of sections taken from the cervical and dorsal regions of the cord of a case that had been under the care of M. Duchenne, M. Charcot arrived at wholly negative results. The white columns were in a state of perfect integrity, and the grey matter exhibited not a trace of alteration. The anterior cornua were neither atrophied nor deformed; the neuroglia presented its normal characters, as did also the nerve-cells. The nerve-roots, both anterior and posterior, were healthy. These results corroborate previous observations by Eulenburg and Cohnheim, and confirm the conclusion that pseudo-hypertrophic paralysis is not dependent upon any appreciable lesion of the spinal cord or nerve-roots.

M. Charcot analyses the record of a case of this form of paralysis by M. Barth, who arrived at an opposite conclusion. M. Charcot observes that the advanced age of the subject, the existence of acute pains in the limbs at a certain stage, the difficulty of speech and deglutition, among other circumstances, are adverse to the admission that M. Barth's case is one of pseudo-hypertrophic paralysis, while they show that it belongs to a category in which the lateral columns of the cord have undergone sclerosis with progressive atrophy of the nerve-cells and anterior cornua. The muscular lesions found in this case M. Charcot admits are such as are usually found in pseudo-hypertrophy, but they are not such as belong specially thereto. The same condition of the muscles may, he adds, be seen in infantile paralysis, as a consequence of traumatic lesions of the nerves, and also in progressive muscular atrophy. The fatty state of the muscles, M. Charcot agrees with Muller, depends upon a general lipomatose state of the body, although he declines, with Muller, to associate the class of muscular atrophic cases with atrophy of the motor nerve-cells. How far this form of paralysis may be attributable to lesion of the sympathetic, M. Charcot adds, must be determined by future researches.

To proceed to the general pathology of this affection, we observe that from the records of 79 cases collected by Dr. Friedreich he deduces several principal points. In the first place he draws attention to the influence of hereditary tendency manifested by their histories, and the predominance, also, in the number of children of the male sex. Thus, in 77 instances (in

4 of which the sex is not distinguished) we have 64 males and only 13 females. Of the latter, 5 belonged to the same family. In another instance of a family of 8 children, 4 males and 4 females, the brothers all suffered, while the sisters escaped from the malady.

The result of the examination of the statistics that have been collected goes to prove that this affection is one belonging to infancy, and is seldom developed in after-life. Thus, of 75 cases it was found 45 times in children under five years of age, while between the ages of eleven and sixteen only 8 cases were met with. Whether it may be a congenital disease is not determined. It is observable that the change commences in those muscles that are most actively employed during infancy and childhood.

The hereditary or family tendency of this form of disease is very plainly pronounced. Thus, of the 81 cases collected by Dr. Friedreich, in 35 instances two or more belonged to the same family. It is obvious that a direct transmission of the tendency from the father or mother is not to be looked for, as the subjects of the affection would necessarily be precluded from marriage. It is also observed here that, as in progressive muscular atrophy, the disease, when it attacked more than one member of a family, commenced at nearly the same age. The only assignable outer influences in predisposing to the disease would appear to be previous chronic disease, insufficient food, inefficient protection from cold and damp.

On the next point in the general pathology of this disease, viz. its localisation, it is found that the muscles which in nearly all cases are the first to manifest the affection are those of the lower extremities, especially the gastrocnemii and solei, reversing the order observed in progressive muscular atrophy. Those of the upper extremities and trunk seldom participate in the disorder. Dr. Friedreich quotes two instances in which it attacked the tongue. Usually the symmetrical muscles or groups of muscles are affected. In one of Duchenne's cases nearly all except the pectoral muscles suffered; even the facial muscles did not escape. In a large proportion of cases atrophied muscles are to be found near those presenting the pseudo-hypertrophic change, exhibiting, as Dr. Friedreich observes, a striking contrast with the increased volume of the adjacent groups. Portions of muscle removed by means of the harpoon show the primitive fibres to be wasted and discoloured, with great increase of fat-tissue and connective. The degree to which these last are augmented constitutes, according to Cohnheim, the difference between hyper- and hypo-voluminous muscles, other qualitative differences being imperceptible.

Dr. Ord has graphically described the physical characters of the disease, and the histological appearances as observed by him in a patient at St. Thomas's Hospital. The following typical case by Dr. Lake well illustrates the clinical history of the series, and may be quoted with advantage in reference to the preceding remarks.

"Andrew B—, aged nine years, a bright and intelligent-looking boy, was brought to the infirmary on November 11th, 1872, on account of weakness of the lower limbs. Attention was at once arrested by his odd gait and by the enormous size of both calves, which were rendered still more conspicuous by the knickerbockers which he wore.

"He was subsequently admitted as an in-patient, and the following is a description of the case:

"The boy being stripped, the gastrocnemii, muscles of the thigh, glutei, extensor muscles of the spine, infraspinati, and rhomboidei are seen to be remarkably increased in size, being firm, hard, and resisting to the touch. The gastrocnemii are those most affected, the body of each cropping out boldly, especially on being put into action. The vastus externus of each leg is also exceedingly prominent, being next in order of the considerably developed muscles; next come the glutei, and then the extensor muscles of the spine, which latter cause the spinal groove to appear much sunken. From the hypertrophy of the oblique abdominal muscles, the abdomen is observed to be highly prominent, but this is due mostly to a considerable anterior curvature in the lumbo-sacral region, and which is shown by a plumb-line touching the spines of the vertebræ at the most prominent point falling far behind the sacrum. Muscles of the head, neck, and chest are not hypertrophied, neither are the muscles of the arm, except that the supinator longus may be a little more prominent and harder than natural, and his grasp is feeble and awkwardly performed. Electro-muscular contractility both of the affected and non-affected muscles unimpaired.

"The integument of the extremities and parts of the trunk presents a curious mottled appearance, purple-coloured patches alternating with white being scattered about, while the backs of the hands and feet are of a bright rosy hue. Cerebral (?) maculæ can be readily and conspicuously produced on most parts of the body. Cutaneous sensibility normal. Temperature (axilla) 98·8° F.

"His gait, as before stated, is very peculiar; it is of a waddling side-to-side character, with limbs unnaturally far apart He makes considerable noise in progression, and is easily fatigued. He has no increased difficulty in walking with his eyes closed.

"In consequence of the weakness of the muscles of the back and legs, he has great difficulty in getting into bed, or standing on a moderately high stool, and for the purpose is obliged to lift one knee with his hand on to the bed, or stool, and then draw up the other leg slowly after him. From the same cause he is totally unable to mount a small rocking-horse. If asked to sit on a low chair, he does

so suddenly, and when rising places a hand on each knee for support, then raises his body with a sudden jerk backwards; but on being told to extend his arms so as not to touch the knees, he can scarcely get up, and, if sitting on the floor, cannot rise at all. It takes him a long time to ascend a small staircase, and he is obliged to cling to the balusters for support. The following are the measurements of the limbs:

Girth of calf of right leg	$11\frac{3}{4}$ inches.
" left "	$11\frac{1}{2}$ "
Girth of thigh at middle right	$14\frac{1}{2}$ "
" " left	$14\frac{1}{8}$ "
Girth of arm at middle of biceps . . .	7 "

He does not complain of either pain or tenderness in the spine, nor of headache or pains or abnormal sensations in the limbs; has never had any serious illness or convulsions. The bones and joints are well formed, with no disposition to talipes. The pupils are equal, and respond normally to light, and no alteration in the fundus oculi is observable with the ophthalmoscope. The physical examination of his chest and abdomen detects nothing abnormal. Other functions normal.

"The weakness in the legs has at all times been noticed, but latterly has become much worse. He was four years old when he first began to walk, and for a long time after was constantly stumbling and falling about, which he does occasionally even now. The increased size of the limbs was not observed till three years ago.

"There is no history whatever of the same complaint in the family, nor of any particular tendency to nervous diseases. He has an only sister, aged twelve, who is perfectly healthy. His mother died eight years ago from some affection of the breast."[1]

The discussion of the next branch of our subject, the relation of pseudo-hypertrophy to other cognate conditions, brings us to the study of the only really cognate or allied condition, viz. that of progressive muscular atrophy. Its complication with osteo-malachia, or other morbid states of the osseous system, being a rare and exceptional occurrence, without direct pathological relationship. Excluding, then, wasting of the muscles from disease, from pressure on nerves, or from fevers, we have to consider, as in the former case, with reference to pseudo-hypertrophy, whether progressive muscular atrophy is to be regarded as a primary idiopathic affection of the muscular fibre or the result of impaired nervous influence—in short, whether we have herein to deal with a neurosis or a myopathy.

Dr. Friedreich here comes to our help, as with indefatigable industry he has compiled an almost exhaustive list of authors,

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[1] 'Lancet,' July 26th, 1873.

whose cases and opinions he has with impartiality set before his readers. Dr. Friedreich gives, moreover, the histories of twenty-four cases that have come under his own observation, in six of which he had the opportunity of examining the pathological changes of the muscles and of the nervous apparatus. Over and above this he has, by the use of Middledorf's harpoon, been enabled to examine portions of muscles removed during life, as well as after death.

Opinions are divided upon the nature and extent of the pathological changes occurring in the muscular tissues in progressive muscular atrophy. By most writers the lesion has been described as a fatty degeneration, by others as a molecular disintegration of the muscular fibrils, and lastly others have attributed the disintegration to inflammation. The description of these changes as recorded by the various writers is fully detailed by Dr. Friedreich. This account we cannot now follow, but must content ourselves by stating that the author regards the lesion of the muscular fibres in this lesion, as in pseudo-hypertrophy, as essentially the result of the influence of disintegration, not simply and originally one of fatty degeneration. The earliest phase of this change consists, according to Dr. Friedreich, in a division or multiplication of the nuclei of the muscular fibres, with a proliferation of the nucleoli. On these follow other changes in the contractile substance, with disappearance of the striation. The histological changes described by the author are amply illustrated by engravings, which obviously must be seen to be appreciated; at the same time, we should add, the detailed descriptions of the microscopical appearances can scarcely be intelligibly transferred to our pages without the accompanying drawings. The following propositions are laid down by the author as the summary of this part of the investigation. Under the name of progressive muscular atrophy we have an essentially inflammatory process affecting the muscular structure, a true *polymyositis chronica progressiva*. The first changes begin in the *perimysium internum* as an hyperplastic increase of the connective tissues. Simultaneously there occur in a greater or smaller number of primitive fibrils the evidences of inflammatory excitement in the form of swelling and increase of the corpuscles and their nuclei, with thickening of the parenchymatous striated substance. With the exception of the interstitial connective, the muscular fibres pass through different phases, partly of simple wasting and progressive breaking up of the transverse striations, and partly of waxy or fatty degeneration. The consequence of these changes is a more or less perfect fibrous degeneration (cirrhosis or sclerosis) of the muscles. The author further compares the

changes he describes with those of other forms of inflammatory affections of the muscles, more particularly those seen in pseudo-hypertrophy and in typhus, and considers that the pathological process is the same in both cases. The outward similarity of the lesions will be strikingly obvious on comparing the illustrations that accompany this work with the drawings given with Dr. Greenhow's paper in the 'Transactions of the Clinical Society,' vols. v and vi. In the next place the author reviews the records of pathological changes in nerves and nerve-centres, in cases of progressive muscular atrophy, and discusses their relation to the latter. The attention of the profession was first drawn to this relationship by Cruveilhier in 1853 and 1856.[1] In two cases related by Cruveilhier the anterior roots of the cervical nerves were reduced to mere neurilemma. In these, as in two other cases reported by Cruveilhier, the condition of the brain and spinal cord is especially declared to have been normal. The same result was pronounced in cases related by Dr. Reade,[2] and by M. Duménil,[3] who both found atrophy of the anterior roots of the spinal nerves, but did not submit the cord to a microscopical examination, without which, *properly performed*, conclusions as to the non-existence of pathological changes are worthless. Vulpian[4] describes atrophy of the nerve-roots in a case in which the spinal cord *appeared* to be healthy. Trousseau[5] mentions instances of atrophied conditions of the anterior roots of the spinal nerves, but adds nothing special as to the state of the spinal cord. Jaccoud[6] also states that, in two cases in which atrophy of the nerve-roots was found, the cord *appeared* healthy under the microscope, a somewhat indefinite statement. A doubt occurs whether in some of the preceding instances efficient modes of examination were adopted to determine so refined and difficult a point. The doubt is, in our opinion, strengthened by the subsequent histories, given by Dr. Friedreich, of many cases in which structural disease of the spinal cord was discovered by the skilful employment of more advanced microscopical manipulation. We quote first Schneevogh's case,[7] a man aged fifty-eight years, who suffered from progressive muscular atrophy of both upper and lower extremities. In this case the fibres of the anterior roots had become so attenuated as to leave no trace of nerve structure. The spinal cord from the fifth cervical to the second dorsal vertebra was softened and

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1 'Archives Générales de Médecine.'
2 'Dublin Quarterly Journal of Medicine,' Nov., 1856.
3 'Gaz. Hebdom.,' 1867.
4 'L'Union Médicale,' 1863.
5 'Clinique Médicale,' 1868, p. 604.
6 'Gaz. des Hôpit.,' 1865.
7 'Wiener Med. Presse,' 1869.

studded with fat-corpuscles. The brachial enlargement was deficient. Valentiner[1] also has mentioned the case of a man in whom atrophy of the nerve-roots had existed together with degeneration in the spinal cord. Luys[2] relates an interesting example in a man who died with progressive muscular atrophy of the upper extremities. The anterior roots of the left side were thin, grey, and friable, while those on the right side were but slightly atrophied. The spinal vessels were in a congested state and surrounded with granules of fatty matter. The fatty corpuscles extended into the grey matter, accompanied with many amyloid bodies. The nerve-cells of the anterior horns were scanty, and on the left side these were in a state of pigmentation, degenerated, and without their processes. Baudimont[3] has recorded a case in which he found an atrophied condition of the anterior columns. Duménil,[4] again, has given the history of a case in which atrophy of the anterior columns had progressed to a great extent. The nerve-fibres were atrophied, and in many parts were destroyed. Between the sounder elements of the cord there was observed an increase of connective tissue, interspersed with amylaceous corpuscles (?) and round or oval granular nuclei. The same appearances are also described in the grey matter. The multipolar cells were few in number, and these shrivelled and wanting their processes, the central canal blocked with débris of cells. These changes extended to the medulla oblongata. The posterior columns were in a normal condition. Schuppel[5] relates a case in which the principal lesion observed was dilatation of the central canal from the upper part of the cervical to the lower part of the dorsal region, displacing and destroying part of the grey matter. Drs. Lockhart Clarke and Radcliffe are also quoted by the author with reference to extensive changes found in the spinal cord in a case of progressive muscular atrophy.[6] Very similar changes are also recorded in a case related by Hayem[7] which had lasted upwards of six years. The anterior roots of the cervical spinal nerves were greatly atrophied. In many fibres the nerve-tubules were broken up into detached portions of granular fatty matter. The grey matter of the cervical region had undergone great changes, and its multipolar cells had for the most part disappeared. Hayem enters into very full

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[1] 'Präger Vierteljahrsschrift,' 1855.
[2] 'Gaz. Méd. de Paris,' 1860.
[3] 'Journal de Bourdeaux,' 1866.
[4] 'Gaz. Hebdom.,' 1867.
[5] 'Archiv der Heilkunde,' 1865.
[6] 'Brit. and For. Med.-Chir. Rev.,' 1862.
[7] 'Archives de Physiologie.'

details of the pathological changes in the nerve substances and in the vessels of this case.

As has already been observed with reference to pseudo-muscular hypertrophy, it would seem that no constant pathological changes of the nervous system are discoverable in progressive muscular atrophy. Wide differences are observable, both with reference to the nature and the extent of the alterations, in the structure of these organs, central or peripheral.

In many of the recorded cases no microscopical examination was made. These cases, therefore, should be eliminated from the discussion, as affording scarcely even negative evidence. It must, however, be conceded, with Dr. Friedreich, that a link in the pathological chain is wanting through the lack of a sufficient number of exact microscopical examinations of the nerve-roots and of the nerves distributed to the affected muscles. Thus, a nerve to external aspect apparently healthy may be the seat of extensive changes, while its neurilemma may be thickened and have undergone alterations discoverable only by microscopical investigation.

The author thus puts forward his view of the pathology of progressive muscular atrophy:

"If we place ourselves for a moment on the stand-point of those who attribute progressive muscular atrophy to a primary affection of the nervous system, we are at once confronted with the question, how is it possible that one and the same myopathy, so clearly and sharply characterised (clinically and anatomically), should be developed as the consequence of alterations so variable in the nervous system as are found in isolated cases? It is scarcely conceivable how alterations of different portions of the nervous system—at one time an affection of the white substance of the spinal cord, sometimes an alteration of the grey matter, in another instance of the roots of the spinal nerves and in some cases of the sympathetic—how all these could be accompanied with a uniform disorder of the muscular system. Furthermore, in the face of all these various lesions of the nervous system, how can we account for those cases of a similar myopathy occurring with perfect integrity of the nervous system? Under these circumstances it would be well to attempt to answer the question in an inverted form—to start from the examination of the muscles, to consider the myopathy as the primary cause, and to assign to these lesions of the nervous centres a secondary importance only. If it be established that the muscular affection may exist in one case with and in another without the lesion of nerve structures, one is justified in the view that it is the essential cause, and that changes of the nervous system do not necessarily or invariably follow" (p. 114).

The pathological process is traced by the author from the muscles to the peripheral nerves, and from these centripetally

to the spinal cord. In support of this view cases are cited in which distinctly degenerative changes were found in the nerves, but were not traceable into the nervous centres. Dr. Friedreich sees special confirmation of his explanation in certain cases in which muscular atrophy existed in both upper and lower extremities, but in which degeneration of only one set of nerves, the upper, was found. In these the possibility of a centrifugal cause would appear to be excluded. The degeneration of the nerves of the upper extremities is consistent with the clinical fact that atrophy of the muscles of the upper extremities usually precedes that of the lower limbs. The cases that are on record in which extensive lesions of the spinal cord were found (*e. g.* by Lockhart Clarke, Gull, Jackson, Swarzenski, Fromman, Virchow, and others) are disposed of by the suggestion that the condition of the muscular nerves and their roots have been insufficiently examined or wholly overlooked. The fact of the more frequent occurrence of lesions in the upper segments of the cord and in the muscles of the upper extremities, although the muscles of lower extremities were also affected, is with some force adduced by the author in support of the centripetal theory. In a case related by Lockhart Clarke and Jackson, and in another by Duménil, the spinal nerve-roots became so rapidly affected in succession, or simultaneously, that the entire length of the spinal cord had suffered alteration of its structure.

According, then, to Dr. Friedreich's view, progressive muscular atrophy begins, in the first instance, like pseudo-hypertrophy, within the muscular tissue as an active inflammatory process, which in its histological characteristics corresponds with other forms of chronic myositis. Sooner or later the inflammatory irritation communicates itself to the intra-muscular nerves, and developes in them a chronic neuritis and perineuritis, which, further, is propagated to the nerve-trunks, and in a centripetal course to the spinal cord. The pathological process may come to a stand at any one stage of its course. The morbid process, beginning in the muscular tissue, connects itself with the motor fibres in the mixed nerve-trunks. Hence it may be conceived that the change in the spinal cord will affect the anterior in preference to the posterior columns.

In like manner the author considers that healthy nerve-fibres, which are found associated with degenerated fibres, belong to the functions of sensation, although in many cases it may be seen that the inflammatory process extends to these also, and by consequence an affection of the posterior columns of the spinal cord follows. An extension also of the inflammatory degeneration is assumed by the author as proceeding from

the posterior roots by the intervertebral ganglia to the sympathetic nerves.

The views of Dr. Friedreich are borne out by two cases of acute muscular atrophy more recently reported by Dr. Greenhow.[1] In both instances the muscles had undergone granular andwaxy degeneration, resembling what is seen in typhus. The changes found in the spinal cord differed somewhat in the two cases, as reported upon by Dr. Cayley and Mr. Kesteven. In one of these the multipolar cells had undergone granular pigmentation, in the other example they had suffered no change in character. In both cases there was an increase in the connective and its nuclei. Similar lesions are also recorded by M. Gombault in a case of muscular atrophy from lead-poisoning.[2] In these the proliferation of the connective tissue and the waxy degeneration were the most prominent pathological alterations.

The preceding extended examination of extant researches into the pathological conditions of these two forms of muscular affection has, we think, sufficiently shown their pathological identity, and will, we think, serve to remove both from the category of neuroses.

With reference to treatment but little can be said that offers encouragement. As the prognosis must be founded upon the number of muscles or groups of muscles affected, so will therapeutic efforts be baffled in proportion to the extension of the malady. Internal remedies have been found useless. The efforts, therefore, of therapeutics must, indeed, be rather directed to prevention than to cure, by the control or direction of the training, regimen, and occupations of members of families in which there may be a known proclivity to this form of disease. In early life all those means should be carefully adopted that are calculated to strengthen the muscles and invigorate the frame. In the event of an early recognition of the affection the judicious employment of induced electricity for a sufficient length of time offers some hope of arresting its progress. Some cases are mentioned by Friedreich in which cures have been effected by galvanism applied to the sympathetic nerves. M. Duchenne states that the disease is curable in its first stage by faradization. The electrical excitability of the pseudo-hypertrophic muscles is, however, greatly diminished, even almost to total extinction, in some advanced instances. This is what might be looked for, seeing that the integrity of the muscular structure is destroyed or impaired in proportion to the acuteness of the inflammatory process

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[1] 'Transactions of Clinical Society,' vols. v and vi.
[2] 'Archives de Physiologie,' Sept., 1873.

or the length of its duration. The choice of the form of electricity to be employed must, therefore, be determined by its effects, carefully watched by persons skilled and experienced in the application of this agent and its therapeutical employment in these cases.

VI.—The Modern Treatment of Stone.[1]

No surgical proceeding has attracted more notice for over two thousand years than the operation for the removal of stone from the bladder; and while the last quarter of a century has especially been noted for the progress made in perfecting the methods of crushing a stone, in the last two years public attention has, from its political relations, been so fully directed to

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[1] 1. *Traité Pratique des Maladies des Voies Urinaires.* Par Sir H. Thompson, F.R.C.S., Professeur de Clinique Chirurgicale et Chirurgien à University College Hospital; Chirurgien extraordinaire de S.M. le Roi des Belges; Fellow of University College; Membre correspondant de la Société de Chirurgie de Paris. Traduit avec l'autorisation de l'auteur et annoté par Edouard Martin, Edouard Labarraque, et Victor Campenon, Internes des Hôpitaux de Paris, Membres de la Société Anatomique. Précédé des Leçons Cliniques sur les Maladies des Voies Urinaires professées à University College Hospital, traduites et annotées par les Docteurs Jude Hue et F. Gignoux. Avec 280 figures intercalées dans le texte. Paris, 1874.

2. *Leçons Cliniques sur les Maladies des Voies Urinaires, professées à University College Hospital de Londres.* Par Sir Henry Thompson, Chirurgien extraordinaire de S.M. le Roi des Belges; Professeur de Clinique Chirurgicale et Chirurgien à University College Hospital. Traduites, annotées et augmentées d'une Introduction Anatomique par les Docteurs Jude Hue, Ex-Chirurgien en chef de l'Ambulance Internationale Rouenaise, Ex-Chirurgien Aide-Major Stagiaire au Val-de-Grace de Paris, &c.; F. Gignoux, Ancien Interne de l'Hôpital Saint Éloi, Ancien Aide d'Anatomie et Lauréat de la Faculté de Médecine de Montpellier, &c. Ouvrage contenant 40 gravures sur bois et 3 Leçons de plus que la troisième et dernière Edition Anglaise. Paris, 1874.

3. *Diseases of the Urinary Organs, including Stricture of the Urethra, Affections of the Prostate, and Stone in the Bladder.* By John W. S. Gouley, M.D., late Professor of Clinical Surgery and Genito-urinary Diseases in the Medical Department of the University of the City of New York; Surgeon to Bellevue Hospital; Fellow of the New York Academy of Medicine; Member of the New York Pathological Society, of the Medical Society of the County of New York, &c. With 103 wood engravings. New York and London, 1873.

4. *A Practical Treatise on the Surgical Diseases of the Genito-urinary Organs, including Syphilis, designed as a Manual for Students and Practitioners, with Engravings and Cases.* By W. H. Van Buren, A.M., M.D., Professor of the Principles of Surgery, with Diseases of the Genito-urinary System and Clinical Surgery, in Bellevue Hospital Medical College; Consulting Surgeon to the New York Hospital, the Bellevue Hospital, the Charity Hospital, &c.; and E. L. Keyes, A.M., M.D., Professor of Dermatology in Bellevue Hospital Medical College, Surgeon to the Charity Hospital, Venereal Division; Consulting Dermatologist to the Bureau of Out-door Relief, Bellevue Hospital, &c. London, 1874.

5. *Address on Surgery delivered before the British Medical Association at Norwich, August,* 1874. By William Cadge, F.R.C.S., Surgeon to the Norfolk and Norwich Hospital.

the subject that we need make no apology to our readers for reviewing the modern treatment of stone, *àpropos* of recent publications in this and other countries.

Before turning to the more strictly surgical part of our subject it will be convenient to consider some modern teaching on the subject of the formation of stone and the influence of diet and medicine upon it. In a lecture added to the last English edition and republished in the French editions of his works, Sir Henry Thompson discusses the early history of calculus from the point of view of the prevention of the formation of lithic-acid calculus. He divides calculi into those of local and constitutional origin, and devotes the lecture to the latter only, of which he says—" Now, from observation we know that nineteen out of twenty of such stones have uric acid for their basis, the remaining one in twenty being oxalate of lime, and, less commonly still, there are phosphatic stones, which are of constitutional origin also. Therefore, practically, to all intents and purposes, the problem before us is how to prevent the formation of uric-acid calculus." Although as regards vesical calculi this estimate is no doubt pretty correct, it must not be forgotten that the majority of large renal calculi are phosphatic, and these must be considered as much of constitutional origin as the small uric or mulberry calculi which find their way down the ureter into the bladder.

Sir Henry Thompson takes up the view expressed, we believe, originally by Dr. Garrod in the 'Medico-Chirurgical Transactions' for 1848, in the following words:—" The results of these experiments on the condition of the blood and urine prove that uric acid is not a product of the action of the kidneys, as frequently supposed, but is merely excreted from the system by these organs,"—and he proceeds to allot to the liver some of the functions often attributed to the kidney, and to place to the account of a " torpid liver" the undue secretion of uric acid. This view of the pathology of the liver has received the support of no less an authority than Dr. Murchison in his recent 'Croonian Lectures,' and is also supported by Dr. Basham in the 'Practitioner' of October and November, 1874, but we must hold, at present at least, that the case is " not proven," though we are not prepared with a specific theory any more than Dr. George Harley, who, in his work on the 'Urine,' says—" The seat of origin of the uric acid in the animal economy is still a question *sub judice*. One thing is, however, certain, namely, that it is not formed by the kidneys."

Mr. Cadge, in his 'Address on Surgery' at Norwich last August, combated very forcibly this hepatic view of the formation of uric acid, and maintained that, " on the whole, it is safer

to attribute lithuria to dyspepsia and malassimilation, which probably concerns all the digestive organs, than to fix the fault mainly on one." Mr. Cadge deprecated the modern tendency to degrade the kidney to a mere excretory organ, and maintained that "the proper function of the kidney was to form as well as to eliminate" uric acid and urea, using as an argument against the circulation of uric acid in the blood the non-occurrence of gout in patients of a decidedly lithic-acid diathesis. On the other hand, it may be fairly argued that the excretion of lithic acid in the urine of patients of this class probably saves them from an attack of gout, for Dr. Garrod, in his work on 'Gout,' expressly states that there is a deficiency of uric acid in gouty urine, and that the defective elimination of that salt bears a close relation to the inveterate character of the malady.

As regards treatment, however, Mr. Cadge may claim Dr. Garrod's support, for he has no hesitation in combating the uric acid in gouty blood by alkalies, lithia, &c., in the same way that the bulk of the profession treat ordinary lithuria. Sir H. Thompson, on the other hand, would use remedies directed solely, as he believes, to the liver, and discards the alkaline waters of Vichy or Vals in favour of those of Carlsbad, Friedrichshalle, and Püllna, which all contain sulphate of soda, combined, in the case of the last two waters, with sulphate of magnesia. His view of the action of these purgative waters is "that they produce activity in all the digestive functions, and thus waste matters, which have been hitherto thrown out as uric acid by the kidneys, are eliminated in some other form." Though not prepared to agree entirely with this view, we most cordially endorse Sir H. Thompson's dictum that there is the greatest possible difference between the therapeutical results obtained by the use of the natural waters and the mere salts which they contain, whether they be obtained by actual evaporation of the same waters or otherwise. The directions he gives for the administration of the waters are very judicious, and are evidently the results of considerable personal experience.

But whether uric acid be the product of the kidney or the liver, it must be conceded that the formation of calculus is governed by local causes, which Mr. Cadge, in the address referred to, has very ably investigated. It is impossible to look at his table, showing the number of stone cases admitted into various hospitals during five years, without being struck by the direct relation of hard drinking-waters to calculous disorders. The Norfolk, Birmingham, and London groups of hospitals, which are supplied with hard water, stand in strong contrast to those of Lancaster, Plymouth, and Taunton, where the water is soft and stone almost unknown. The water of Norfolk

averages thirty-three degrees of hardness, according to Clark's scale, or more than twenty degrees above the average, and can hardly fail to have an effect upon the animal economy, though what the precise action may be is, and probably will be, disputed. Mr. Cadge quotes with approval Dr. Prout's dictum, "that hard waters, in conjunction with other favorable circumstances, have a great influence in producing stone; they derange delicate stomachs very considerably, and have a tendency to produce lithic sediments, though they generally act by causing a deposit of the crystallized sediments or gravel in those disposed to them;" but he notes the fact that both lime and lithic acid are exceedingly insoluble, and that there is no chemical affinity between them, so that, in fact, a calculus of lithate of lime is unknown. Mr. Cadge hazards the suggestion that "the constant filtration in excess through the system may encourage the separation and deposit of lithic acid in the urinary passages," but he demurs altogether to the notion that hard water favours the oxalic or phosphatic diathesis. On the other hand, Dr. George Harley states that "oxalate of lime and phosphatic calculi are the two most common forms of stones met with in districts where the water is impregnated with large quantities of lime," and his views have been corroborated by Dr. J. C. Murray, of Newcastle, who, two years ago ('British Medical Journal,' 28th September, 1872), called attention to the subject, and has recently pursued it in the columns of the 'Medical Times and Gazette.' Dr. Murray believes that lime in drinking water may lead to the formation of any calculus except the xanthic oxide, and says, "not only may lime be mixed with or coat other nuclei, but even in uric-acid deposits, which have been thought to be benefited by lime in water, the lime, by uniting with the free hydrochloric acid in the stomach, will leave soda at liberty to unite with uric acid, causing rheumatic gout, and also the chalk stones which we so frequently see." Dr. Murray gives cases which appear to support his views, and at all events the practical "outcome" of his researches has been the formation of the "Northumberland and Durham Pure Water Company," for supplying aërated distilled water, and it will be interesting some years hence to know whether thereby the occurrence of calculous and gouty disorders has been diminished in Newcastle.

The formation of calculus naturally leads to the question of its re-solution when found, and this was the subject of a lecture by Sir Henry Thompson, which appeared in the 'Lancet' of April 3rd, 1873, and has been added to the French edition of his works. Traced in chronological order from the time of Pliny to that of Mrs. Stephens, we have the

result thus tersely summed up in the lecture referred to:—"We are now in a position to arrive at the following conclusion, the only one possible, viz. that all the quack and would-be secret medicines employed from time immemorial to the present day are solutions of either lime, soda, or potash, alone or combined." Dr. Roberts, of Manchester, has shown that carbonate of potash is the best solvent for uric stones, and believes that, under favorable circumstances, the long-continued administration of the drug may so dissolve the outer layers of a small calculus as to admit of the passage of the nucleus *per urethram.* In renal calculi, more as a preventive of further growth, as recommended by Dr. Basham, than as a solvent, carbonate of potash is doubtless a useful remedy; but, given the suffering of a vesical stone, we imagine most persons would agree with Sir H. Thompson in preferring more active surgical treatment.

In cases of phosphatic calculi, or rather for phosphatic concretions forming constantly in unhealthy bladders, Sir Henry Thompson strongly recommends the use of acid injections, or a solution of acetate of lead to be thrown into the bladder, and in these cases we have seen benefit result from restricting the patient to the use of distilled water as a drink. Dr. Harley speaks highly of the treatment of such cases by a continuous current of acidulated water, flowing through a double catheter into and out of the bladder, and the late Dr. Bence Jones combined the use of the electric current with solvents, but neither plan has come into general application.

Before entering upon the purely surgical methods of ridding the bladder of a stone, it will be well to discuss some very important points raised by Sir H. Thompson, in a lecture now first added to the French edition of his works, but which appeared in the 'Lancet' of March 8th, 1873, soon after the Emperor Napoleon's death, and was generally regarded, rightly or wrongly, as that eminent surgeon's 'Apologia.' In this lecture the influence of renal disease on the choice of operation for stone in the bladder was fully discussed, and the question of diagnosis investigated. Sir Henry Thompson repudiates with warmth the term "surgical kidney," as implying a slur upon his craft, but this is the form of disease which proves fatal to so many cases of stricture and stone, and which consists, essentially, of a dilated ureter and pelvis and more or less pyelitis, with abscesses dotted about the cortical substance of the kidney, and lines of pus in the pyramids. This is but the later stage of long-standing disease due to irritation of the urinary organs, the kidney being in a chronically inflamed state, which requires but a slight additional stimulus to light up into acute disease. The important ques-

tion is, can this dormant condition be diagnosed? and Sir H. Thompson says very plainly it cannot, and in this opinion he is confirmed by the anonymous author of an able article on "Surgical Kidney" which appeared in the 'Lancet' of January 18th, 1873. On a matter of such great importance we need not apologise for quoting the lecturer's own words. He says:

"I have long sought for some sign that should indicate the presence of extensive pyelitis and dilatation, but in vain. Such a patient presents no sign of change in the urine itself. It is of full specific gravity, abundant in quantity, without albumen, except that which the presence of pus and blood accounts for, and such pus and blood are commonly found as vesical products formed by irritation from the calculus in cases where no renal disease exists. Whenever you have stone of more than small size you may have such products, and we are bound to expect them if the patient has any symptoms of cystitis, and some cystitis is always present in these cases of dilatation. Again, there is nothing which we can identify as disintegrated portions of renal tissue. No casts of tubes, nothing but pus and blood-discs—nothing, in short, distinctive. Then at no stage of the disease is there any dropsical effusion, no habitual dryness of the skin, not necessarily any marked feverish state, constant or intermitting. Nor is there any diminution of weight; on the contrary, the patient may have gradually acquired fat, but he is always, if the condition is advanced, in feeble health, is worn, and easily exhausted—signs which impress you with nothing so much as his obvious inability to bear any severe test of his physical powers, from all of which, however, nothing absolutely diagnostic can be inferred."

Van Buren also says in effect much the same at p. 278 of his work on the 'Urinary Organs,' from which we take the following:

"During life the existence of these serious complications cannot be made out with any absolute degree of certainty; habitual tenderness on deep pressure over the kidneys, tendency to chill on slight provocation, increased frequency of pulse towards evening, nausea and capricious appetite, with feeble digestion, and similar evidences of failing health, which cannot be otherwise adequately explained, are symptoms from which the existence of these lesions may be inferred. Any operation undertaken upon a person in this condition is liable to be followed by rapidly fatal symptoms, due, most probably, to uræmia."

Of course, patients suffering from the ordinary form of Bright's disease, with urine of low specific gravity and loaded with albumen, may be sufferers from stone, and Sir H. Thompson gives three instances of the kind in the lecture from which we have already quoted. All three were successfully treated by lithotrity, and Sir Henry does not hesitate to say that they

would not have borne lithotomy, and believes that modern lithotrity in skilful hands offers a better prospect to the other class of renal cases than any cutting operation. This is not the opinion of other surgeons of great experience and success, as, for instance, Mr. William Coulson, who distinctly affirms that "lithotomy holds out greater chance for relief in these unpromising cases"; but, on the other hand, it must be conceded that, as remarked by Thompson, "the lithotrity of to-day is a better and safer proceeding than that of twenty or of ten years ago, and thus it is that the axiom about renal disease, right as it might have been then, has been growing less so year by year."

As regards the diagnosis of calculus, but little improvement has been made of late years, if we except the introduction of the short-beaked sound in place of the older instruments of large curve. Thompson's cylindrical handle is a convenient though not essential addition to the instrument, but his graduated scale and slide are of little practical utility in measuring the size of a stone. The lithotrite offers the only accurate method of determining the exact size of a calculus, but its introduction and the manipulation necessary for grasping a good-sized stone are apt to create more disturbance than is advisable, and a sufficiently accurate estimate can be formed with the sound. The endoscope of Desormeaux, or the modification of it by Cruise of Dublin, is of no practical utility in determining the size of a stone, since only such a minute portion can be brought into view at one time as to be worthless. The ingenious sound tipped with pewter proposed by Mr. Donald Napier also can give but little extra information to the practised surgeon, whilst the sounding-board of Mr. Charles Brooke, though useful for demonstrating the presence of stone to a class, is cumbersome and unnecessary for the operator himself. Stones are, no doubt, occasionally overlooked nowadays, as of old, but the importance of the early recognition of the complaint has been so strongly impressed upon the profession by Thompson in all his writings, as well as by others, that more attention is paid than formerly to conditions of urine, &c., possibly indicating calculus. In his twelfth lecture ("Address on the Future of Operative Surgery for Stone in the Bladder," delivered before the Midland Medical Society, 'Lancet,' 20th December, 1873) he recommends physicians to learn the use of the sound, and condemns by implication those who do not send their patients soon enough to the surgeon to be sounded. But even a surgeon is occasionally misled by the slight inconveniences complained of by a patient suffering from stone, though probably few of the present day would be content to "rest and be thankful," leaving a recognised stone to

grow in a patient's bladder after the manner inculcated a few years ago by a professor of the Royal College of Surgeons of England. On the other hand, cases occur from time to time in which an operation is undertaken without a stone being discovered, and Mr. Gutteridge is to be especially noted for his boldness in having published three cases of the kind which have occurred in his own practice, and for giving what appears to be the real solution of the accident, viz. the ease with which, in children, the pelvic wall can be struck and made to emit a sound closely resembling the "click" of a stone. No doubt in some cases of unsuccessful lithotomy a stone is really present, but, as pointed out by Sir Wm. Fergusson, the surgeon never reaches the bladder, making a pouch outside of it with his finger; and it may be noted as a fact within our personal cognizance that, a year or two back, within one week, at different metropolitan hospitals, two boys were cut without the discovery of a stone, and a third boy, who had been placed on the table for operation, was sent away without a stone being found sufficiently obviously to warrant the performance of lithotomy!

To come now to the question of choice of operation. Thompson has done much, both in this country and abroad, especially in America where his works are extensively read and quoted, to popularise lithotrity and make it take the place of lithotomy. No one who has seen him operate can doubt his great skill and dexterity and the perfection of the instrument he employs. The cylindrical handle combined with the connecting and disconnecting button give the operator greater control over the movements of the blades than was possible with any of the old screw lithotrites, whilst the possibility of instantaneously locking the fragment when grasped is a great advantage over Fergusson's simple rack and pinion. Thompson has, we believe, entirely or almost entirely abandoned the fenestrated blade, and ordinarily employs only one instrument with flat scoop blades, the male smaller than the female, so as to avoid any possible pinching of the coats of the bladder. By this means great lightness has been attained, an object so much striven for in all modern lithotrites that it was with considerable surprise we recently saw Mr. Teevan's advocacy ('Lancet,' October 31st, 1874) of heavily handled lithotrites, apparently possessing no special advantage over Thompson's instrument, which it is Mr. Teevan's *rôle* to decry from every point of view.

The old or English method of employing the lithotrite, *i. e.* of depressing the female blade to the fundus of the bladder and bringing the stone down to it, is still employed by the older English surgeons, and especially those who venerate the teaching of Brodie; but Civiale's or Fergusson's method, of taking

the lithotrite to the stone and systematically over the whole area of the bladder, has such obvious advantages that it is adopted universally in America and the Continent, and by the younger surgeons here. Thompson's description of this and his general directions for the operation, whether in his work on 'Lithotomy and Lithotrity,' or his more colloquial 'Clinical Lectures,' are so good that they have been transferred bodily to more than one recent surgical work, and we need not repeat them here.

A point much insisted on by Thompson we incline to regard as one of the greatest improvements of modern lithotrity, and that is the abandonment of all washing out of the bladder, both at the time of the operation and afterwards. To have a patient standing up shivering in a draughty operating-theatre, while his bladder was washed out immediately after lithotrity, was one of the most painful sights we ever witnessed, and it led to the surgeon's doing a great deal more than was wise or even justifiable in order to bring away the *débris* before his class. Still more dangerous, we believe, was the practice, inculcated by a great authority, of dragging fragments along the urethra, with the object of hastening the cure. Thompson's practice, as laid down in his 'Clinical Lectures,' of keeping the patient on his back whilst passing water soon after lithotrity, is, we believe, a most important one, since the fragments thus get smoothed off, and are much less liable to become impacted in the urethra. The surgeon's object, however, should be to make his patient pass *sand*, not *fragments* of calculi, and with the careful use of the flat-bladed lithotrite this can be readily attained. Clover's bottle is a useful instrument in some instances, but is by no means of universal application, and in some cases the india-rubber bottle does not appear to have power enough to effect its object. In such cases of atony of the bladder the use of the exhaustive syringe may be advantageously had recourse to.

Thompson's undeniable success in lithotrity may, perhaps, make his teaching with regard to it a little enthusiastic, but there is no doubt that for medium-sized and small stones it is the best operation in adults with a fairly healthy urethra and bladder, and that the danger of the proceeding increases directly as to the size of the stone. No doubt many stones ought to be discovered much earlier than they are, but we cannot say that we are as sanguine as Sir Henry himself in anticipating the proximate abolition of lithotomy.

As regards the success of lithotrity, we may quote the statistics of Dr. Gouley, who has collected the cases recorded by Brodie, Crichton, Fergusson, Thompson, Keith, Ivanchich, Swallin, Porta, and Buck, the aggregate being 992 cases with

101 deaths, or one death in 9·82 cases. This is, however, a higher average of deaths than that obtained by individual British operators, Keith having only one death in 16·57 cases.

Under the name of "Perineal Lithotrity," we have in Dr. Gouley's work a description of an operation devised by Professor Dolbeau, of Paris, by which the membranous portion of the urethra is opened, the prostate and neck of the bladder dilated instead of being cut, when a large stone can be crushed and the fragments extracted at the same sitting. Dolbeau's dilator is composed of six branches, which can be separated with a screw to a circumference of twenty millimètres (thirteen sixteenths of an inch), which is the extent to which M. Dolbeau has found (contrary to the views of most English anatomists and surgeons) that the neck of the bladder can be dilated without tearing the mucous membrane and prostatic tissue. Three stages of dilatation are made, the last including the neck of the bladder, and the stone is then broken up with powerful lithoclasts and extracted piecemeal, the bladder being carefully washed out to remove any *débris*. The advantages and accidents of this proceeding are thus summed up Dr. Gouley:

"The advantages claimed for the operation are—that the situation of the small wound exposes the patient to very little risk of hæmorrhage; there is less suppuration, and the patient is less exposed to its bad effects, and, in fact, that it possesses all the advantages of median lithotomy without its disadvantages. M. Dolbeau urges that perineal lithotrity has not been with him an operation of choice, but of necessity; that he has resorted to it only when lithotripsy by the natural route has been contra-indicated, and where the larger incision in perineal lithotomy might prove serious.

"Among the accidents of perineal lithotrity he once observed complete retention of urine, a circumstance which went to show that the vesical neck had not lost its power of contraction. He states that pain in the hypogastrium and vesical and rectal tenesmus have not been unfrequent consequences of the operation, the vesical tenesmus in some cases having been obstinate and annoying. He has never had to contend with severe bleeding. Rigors have sometimes occurred within the first few hours, rarely later. The supervention of a chill in three or four days may be regarded as a bad omen, perhaps the precursor of pyæmia. He has never had a case followed by a permanent fistula. The wound has usually healed in from two to three weeks."

Dolbeau has had thirty operations with five deaths, and Gouley three cases, all of which recovered. At present, so far as we know, the operation has not been attempted in Great Britain, and its difficulty and tediousness would prevent its general adoption as a substitute for ordinary lithotomy, though

it may prove of service in the case of exceptionally large stones.

Lithotomy will probably always be resorted to for stone in children and for large stones in the adult. It is true that stone among children of the poorer classes is extremely common, whilst among the upper it is almost unknown; and whether Mr. Cadge's theory as to the absence of good milk as a part of their diet may be accepted or not as the cause of this tendency among the poor, the fact must be borne in mind since it accounts for the large number of lithotomies and their success in public institutions. No statistics of lithotomy are of the least use for purposes of comparison with those of lithotrity unless they apply solely to adult patients in both cases, and the figures occasionally put forth by young surgeons undergo woeful alteration when brought to the test of each patient's age. As Thompson remarks in one of his clinical lectures—

"A bold, well-performed lateral lithotomy is quite possible to the young surgeon at the outset of his career, while nothing but considerable experience can make him a good lithotritist. The two operations can never be compared nor their capabilities estimated without keeping in view this fact."

This may probably account for the fact recorded by Mr. Charles Hawkins in his paper on "Lithotrity" ('Holmes,' vol. iv), that in all the general hospitals in London in one year there were but thirty-two cases of lithotrity, while there were fifty-three of lithotomy in the *adult* in the same period.

The lateral operation of Cheselden still holds its own, notwithstanding that from time to time trials have been given to various modifications. Thus, a few years back Sir William Fergusson cut one or two patients by a transverse or semicircular incision in front of the anus, and Sir Henry Thompson cut a few by Dupuytren's medio-bilateral method, using the double *lithotome-caché,* but their results were not sufficiently encouraging to lead others to follow the example.

Allarton's median operation was extensively tried for some few years by many surgeons, but it was found that, except for small stones, the room it afforded was too small save at the expense of laceration of the prostate. Mr. Cadge, whose experience reached ninety cases, thus tersely sums up the case against median lithotomy ('British Medical Journal,' October 11th, 1873):

"If, then, it be admitted that the median operation is unsuited for children—that for the average of cases the mortality is as great as in lateral lithotomy—that the operation is more difficult—that hæmorrhage is more troublesome, and wound of the rectum more

frequent than after lateral lithotomy—it will naturally be asked, what are the advantages of the central operation, and to what cases is it applicable?"

Mr. Cadge's reply is, for "stones not larger than a filbert," in whom for some reason lithotrity is not advisable; and we would add for the removal of foreign bodies, catheters, &c., which cannot be safely extracted *per urethram*.

Buchanan's rectangular staff has been adopted by some surgeons who have not adopted his special operation, but have employed the staff in the ordinary lateral operation on the ground that it is easier and safer to run the knife along a straight guide then around a curved one. This we believe to be an error, as we shall proceed to show later on. The rectangular is more difficult to introduce than the curved staff, and though its angle forms a more prominent guide in the perinæum it is apt to push the bulb forward and endanger its safety. Operators differ a good deal as to the point at which they enter the knife, and we believe that much of the hæmorrhage which characterises the lithotomies of one distinguished living operator is due to the knife being entered too high, so as to cut the artery of the bulb, if not the bulb itself. An inch in front of the anus in the adult is, we believe, quite far enough forward, and a good deep plunge of the knife, completely clearing or even opening the staff, may be safely made at this point with advantage and great saving of time. It is the deep incision which has always been the bugbear of young operators, and to obviate or facilitate cutting on a curve both Mr. Key's straight staff (which is always employed at Guy's Hospital), Buchanan's rectangular staff, and also the peculiar "roughened groove" of Mr. Thomas Gutteridge, were devised. The fact is that, so long as the point of the knife is directed upwards in the groove of the staff, as generally taught, it is impossible to make the knife follow the curve of the staff without a series of movements of the hand and knife which are very apt to displace the point of the latter and send it by the side of the staff into the front of the bladder. But when the point of the knife is once fairly in the groove, if the handle of the knife be raised the point will glide round the curve without difficulty, and with ordinary care can be safely carried to the end of the staff. Mr. Gutteridge's roughened groove enables the operator to feel his way step by step towards the bladder, but this could be more certainly, though less showily, done by placing the finger *beneath* the knife and thus pressing it into the groove. It may be remembered that in 1856 a controversy was raised by Sir W. Fergusson as to whether this was or was not Liston's method of holding the knife, and the weight of evidence went to show that, though

not his later method, it probably was his plan in his earlier cases, though we cannot agree with the late Mr. Syme in his appreciation of "the extreme danger of running a knife along a staff into the bowels of a living man without being guarded by a finger to press it *into* and not *out* of the groove."

The employment of the forceps shows the dexterity of the surgeon more than the cutting portion of the operation. Any one who has seen Sir William Fergusson operate will recognise the truth of the description given by Druitt when he says, "At the moment of entering the bladder Sir W. Fergusson adroitly opens the forceps, and with a turn of the wrist, giving a *scooping* motion to the open blades, catches the stone as it is brought within their jaws by the gush of urine which escapes." Much harm is done at this stage by the forcible dilatation and closure of the forceps with *both* hands by unskilful operators, and we have known at least one case in which the forceps were thus thrust through the bladder into the peritoneum.

The size of the opening to be made into the neck of the bladder has been, and probably will yet be, disputed among surgeons, as also the question of cutting or dilating the prostate. As we have already mentioned under the head of perineal lithotrity, Dolbeau maintains that it is possible to dilate the prostatic urethra to the extent of $\frac{13}{16}$ths of an inch without lacerating the mucous membrane, but Professor Ellis long ago showed that such an amount of dilatation without laceration of the prostatic tissue was impossible, and most English authorities agree with him. The free division of the prostate in children has proved so entirely devoid of danger that surgeons have recovered from the fallacious teaching of Liston as regards the danger of division of the capsule of the gland, and now divide rather than tear the prostatic tissue. Mr. Teevan has very rightly insisted upon the necessity for free, and if necessary repeated, division of the tissues in order to extract large calculi with safety, and the four cases communicated by him to the Clinical Society ('Transactions,' iv) are good examples of the success of the method. We do not agree, however, with Mr. Teevan in his statement that sterility constantly follows lateral lithotomy, even in children, from tearing of the prostate.

We have thus endeavoured to lay before our readers the most modern views on the pathology and treatment of stone, and we may venture to hope with Sir Henry Thompson that, as our knowledge of dietetics and therapeutics improves, stone in the bladder may become so rare that the young surgeon will have to "gain his spurs" in some other field than that of lithotomy.

VII.—The Plea of Insanity.

Philosophers who assert that there is a periodicity in all things may include in their category the periodical convulsion or excitement, call it by what name we will, which passes over society from time to time on the subject of insanity and the responsibility of the insane. As in many other convulsive disorders, the longer the interval the worse is the convulsion. A generation has passed away since the great outcry arose on the occasion of the acquittal of Macnaughten, but the names of Windham and Townley are still fresh in the minds of most readers, and but a short time has elapsed since the press teemed with articles of every shade of opinion on the merits of the Rev. Mr. Watson and Miss Christiana Edmunds.

Not in the heat of excited controversy, but in the interval of tranquil reflection, is it well to consider the plea of insanity and the responsibility of the insane. It is a matter about which doubt and difficulty will probably for ever exist, and this for various reasons. First and foremost, the cases in dispute are for the most part doubtful cases, occurring in persons who are not unquestionable lunatics, but who dwell on that wide borderland between sanity and insanity peopled by so many unfortunates ever drifting from one verge to the other. Who does not know the difficulty of dealing with these? Secondly, the various writers on the subject approach it from totally different stand-points. There are, broadly speaking, three classes—the lawyers, who lay down legal tests applicable to all persons alike, under which are to be brought the insane like any others; the doctors, whose test is a departure from the laws of health, mental or physical; and the press or society in general, whose aim is to protect itself by the punishment of evil doers, and by jealously defending its rights and liberties when threatened. Another reason is that the mass of writers—all, in fact, except some few doctors—have no experience whatever of the ill-fated people concerning whom they are writing, and hence some of their productions are as purely an effort of imagination and

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[1] 1. *Special Report from the Select Committee on Homicide Law Amendment Bill; together with the Proceedings of the Committee, Minutes of Evidence, and Appendix.* 1874. (Ordered by the House of Commons to be printed.)

2. *Responsibility in Mental Disease.* By Henry Maudsley, M.D. London, 1874.

3. *On the Scientific Value of the Legal Tests of Insanity.* By J. Russell Reynolds, M.D., F.R.S. London, 1872.

4. *Insanity in its Relations to Crime.* By William A. Hammond, M.D. New York, 1873.

fancy as would be the physiology of the Lernean Hydra or the embryology of the cockatrice's egg.

Another difficulty which meets us when we approach the subject is the want of legal precision which distinguishes it. Although the Legislature has by numberless Acts of Parliament regulated the care and custody of lunatics and the mode of administering their property and estates, it has carefully avoided any definition of insanity or of those who are insane, and throws no light upon the responsibility or irresponsibility of persons of unsound mind. The law of insanity of which the lawyers talk so freely is not to be found in the statute book, but is that unwritten law which presents so many charms to the legal mind, and is to be found in the interpretations and decisions of the illustrious men who have adorned the bench in past ages. Because this is unwritten and not statute law the Legislature has great difficulty in dealing with or altering it, and lawyers prefer it because it gives them a certain latitude in the interpretation thereof, and does not tie them to a strict literal enactment. Mr. Justice Blackburn, in his evidence before the select committee on the Homicide Law Amendment Bill, of which more hereafter, says that he is very sceptical indeed of the advantage of what is commonly called codification.

"I am very sceptical, indeed, whether you can find it easy to reduce the law to a proposition; and if you do I think it will be better not to lay down a rigid definition, because you are sure to leave something out which you ought to have brought in, or to bring in something which you ought to have left out. It is much better, it seems to me, to have the elasticity of the common law, and to apply the general principle to the particular circumstances."

What the difficulties are at present in arriving at the law of such subjects as do not come under statutory enactment may be gathered from Baron Bramwell's evidence before the same committee.

"If," he says, "you had to look for a definition of murder, you would not find it anywhere precisely laid down; you would have to search through 'Coke's Institutes,' 'Hawkins's Pleas of the Crown,' 'Leach's Crown Law,' 'Russell on Crimes,' and a score of other books."

If this is the case with regard to homicide it is not to be wondered at that the subject of insanity is still left in the vagueness and uncertainty, or, according to Justice Blackburn, the elasticity, of the common law. There have been no enactments upon it, and yet we shall find that changes have taken place in the interpretation of the law, changes which are due to

public and scientific opinion rather than to judges, to public opinion which judges have been compelled to recognise, though from time to time they still make an effort to show that the law is above opinion, and, like that of the Medes and Persians, altereth not. Dr. Maudsley has well shown how this change of judge-made law has developed. According to Lord Hale—

"There is a partial insanity and a total insanity. The former is either in respect to things, *quoad hoc vel iliud insanire.* Some persons that have a competent use of reason in respect of some subjects are yet under a particular dementia in respect of some particular discourses, subjects, or applications; or else, it is partial in respect of degrees; and this is the condition of very many, especially melancholy persons, who for the most part discover their defect in excessive fears and grief, and yet are not wholly destitute of the use of reason; and this partial insanity seems not to excuse them in the committing of any offence for its matter capital; for, doubtless, persons that are felons of themselves and others are under a degree of partial insanity when they commit these offences. It is very difficult to define the invisible line that divides perfect and partial insanity; but it must rest upon circumstances, duly to be weighed by judge and jury, lest on the one side there be a kind of inhumanity towards the subjects of human nature; and on the other side too great an indulgence given to great crimes;" and, as Dr. Maudsley remarks, "the invisible line, which it was so difficult to define, was not, let it be noted, between sanity and insanity, but between perfect and partial insanity. It was not thought inhumanity towards the dejects of human nature to punish as a fully responsible agent a person who was suffering from partial insanity, whatever influence the disease might have had upon this unlawful act."

The law is held to be the same now as when Lord Hale wrote in the seventeenth century that partial insanity is held to be no excuse; total insanity alone renders a criminal irresponsible. But the determination of what is the degree of insanity to be called partial has been a constant matter of difficulty, and the test has varied from time to time, the test being laid down, be it observed, not by the Legislature, but by the judges, with the latitude which must necessarily attend the enunciation of an unwritten theory as to responsibility. Thus, in 1723 Mr. Justice Tracy lays down what Dr. Maudsley calls the "wild beast" form of the knowledge test. At the trial of Arnold, an undoubted lunatic, for shooting Lord Onslow, he said—

"It is not every kind of frantic humour, or something unaccountable in a man's actions, that points him out to be such a madman as is exempted from punishment; it must be a man that is totally deprived of his understanding and memory, and doth not know what

he is doing no more than an infant, than a brute or a wild beast; such a one is never the object of punishment."

Under this theory during the remainder of the eighteenth century many lunatics were hanged. We need not be surprised at this when we reflect upon the penal laws of that period, when every felony was met by capital punishment, and people, even children of tender age, were executed for the most petty thefts.

In 1812 Bellingham was tried for the murder of Mr. Spencer Perceval, whom he shot under the influence of insane delusions. His trial was hurried on with indecent haste; the Chief Justice Lord Mansfield refused to postpone it, though affidavits were made that important evidence as to his insanity could be procured from Liverpool, and in seven days from the commission of the crime he was executed. Lord Mansfield on this occasion concurred in the opinion of the Attorney-General, who said—

"Upon the authority of the first sages of the country, and upon the authority of the established law in all times which has never been questioned, that although a man might be incapable of conducting his own affairs, he may still be answerable for his criminal acts if he possesses a mind capable of distinguishing right from wrong."

It was not at this time necessary that he should be totally deprived of his understanding and memory as an infant or wild beast, but he must be incapable of distinguishing right from wrong. The test had changed with the elasticity which is the advantage of the unwritten law. Thirty years elapsed and Macnaughten was arraigned for the murder of Mr. Drummond. The man's insanity was evident, and before the close the Lord Chief Justice Tindal stopped the trial and directed an acquittal on the ground of insanity. Then arose an outcry throughout the length and breadth of the land, and it was thought that the law was not too harsh, but too lenient towards such lunatics, and the Lords were importuned to bring in a bill to alter it, the better to secure the punishment of such offenders. The Lord Chancellor Lyndhurst told the House, however, that the law wanted no alteration, and deprecated fresh legislation on the subject, and in the result the Lords left the general law where it stood, but put certain questions to the whole bench of judges as to the application of the law and the way in which evidence might be taken from witnesses. And after due consideration the answers were read to the House, the judges being unanimous with regard to them with the exception of one. We have not space to give these questions and answers in full. To use Dr. Maudsley's words the substance of them is that—

"To establish a defence on the ground of insanity it must be clearly proved that at the time of committing the act the party accused was labouring under such a defect of reason from disease of the mind as not to know the nature and quality of the act he was doing, or, if he did know it, that he did not know he was doing what was wrong. The question of right and wrong in the abstract was here abandoned, being allowed quietly to go the way of the wild beast form of the knowledge test; the question of right and wrong was to be put in reference *to the particular act at the time of committing it.*"

But they limited the application by holding that if a man acting under a delusion commits a homicide, and if the motives concerning which he holds the delusion would not, had they been real facts, have justified such an act, he is responsible, thus prejudging the case instead of leaving the question to the jury.

These answers have been looked upon until the present time as an authoritative exposition of the law as it affects the insane, and juries are told that they must abide by this interpretation whatever their own feelings may be. The consequence is that it generally happens that insane homicides are for the most part found guilty of wilful murder and sentenced to be hanged, and then the Home Secretary revokes the sentence, and more or less opprobrium is hurled at him for so doing. Judges, of course, differ to some extent as to the severity with which they try to press the law against the accused, and some individuals are so palpably mad that the jury in the teeth of the judge's direction acquits them on the ground of insanity. Judges are sometimes fain to apologise for the state of the law which they enunciate, and they remind the jury that they are not responsible for the law; that it is their business to administer it and not to make it, forgetting that there is no enactment in the statute book on the subject, and that the law is judge-made law, which the judges, we presume, can unmake as well as make. That some of them are sensible of its defects we shall see. Meanwhile we are glad to find that the judges of other countries condemn them, and we will adduce the opinion of an American judge,[1] who, after reviewing the answers of the English judges, thus speaks of the doctrine contained in them:

"The doctrine thus promulgated as law has found its way into the text-books, and has doubtless been largely received as the enunciation of a sound legal principle since that day. Yet it is probable that no ingenious student of the law ever read it for the first time without being shocked by its exquisite inhumanity. It practically holds a man confessed to be insane, accountable for the exercise of the same reason, judgment, and controlling mental power, that is

[1] Judge Ladd, *State* v. *Jones*, quoted by Dr. Maudsley, op. cit., p. 99.

required in perfect mental health. It is, in effect, saying to the jury, the prisoner was mad when he committed the act, but he did not use sufficient reason in his madness."

Seeing that we have now arrived at a state of things when trials in which the plea of insanity is urged are so uncertain that, as Dr. Maudsley says, "the issue might as well be decided by tossing up a shilling;" it has seemed time that some action should be taken, and in the last session of Parliament a bill was brought in by the learned Recorder of London to amend the law of homicide. The bill was referred to a Select Committee, who reported against its being passed because it is not desirable to codify the law of homicide by itself, or to adventure on so important a law as a test or experiment. But they—

"Recommended that the attention of the Government and of Parliament should be directed to the present imperfect state of the definition of the law of murder. They believe that they have collected materials from which a redefinition of murder can be produced, and they are convinced that such a definition is urgently needed, not only to rescue the law from its present discreditable state, but to give clear notions to the public at large of the real nature and extent of this crime, and to prevent the confusion often created in the minds of jurors by an appeal to the doctrine that murder cannot be without malice aforethought, which it is not always easy for the judge to remove. If there is any case in which the law should speak plainly, without sophism or evasion, it is where life is at stake; and it is on this very occasion that the law is most evasive and most sophistical."

Thus strongly do the committee speak of the present state of the law. It will not be without advantage to see how the bill proposed to deal with the subject of insanity. In Part II, § 24, it is proposed to be enacted that—

"Homicide is not criminal if the person by whom it is committed is, at the time when he commits it, prevented by any disease affecting his mind—

"(*a*) from knowing the nature of the act done by him;
"(*b*) from knowing that it is forbidden by law;
"(*c*) from knowing that it is morally wrong; or
"(*d*) from controlling his own conduct.

But homicide is criminal, although the mind of the person committing it is affected by disease, if such disease does not, in fact, produce some one of the effects aforesaid in reference to the act by which death is caused, or if the inability to control his conduct is not produced exclusively by such disease.

"If a person is proved to have been labouring under any insane delusion at the time when he committed homicide it shall be presumed, unless the contrary appears or is proved, that he did

not possess the degree of knowledge or self-control hereinbefore specified."

The bill was referred to a select committee, who examined three witnesses, Mr. Fitzjames Stephen, Q.C., who drew the bill, Mr. Baron Bramwell, and Mr. Justice Blackburn. We think that it will be neither uninteresting nor unprofitable to examine their evidence upon the clauses of the bill which have just been quoted.

Mr. Stephen first dwells upon the present state of the law of homicide.

"The reasons why I should wish to see the English law codified are of this character:—I think that the practice of the law, and what I may call the unexpressed understanding upon which the law is administered in the criminal courts of England, is very much in advance of the actual law itself. I think, in fact, that the practice has greatly outrun the theory, and that the theory ought to be brought up to the practice. I do not think it would be true to say that the English law upon homicide at the present day can practically be regarded as in a state of uncertainty. The cases decided by the Court of Crown Cases Reserved do not indicate any uncertainty in the definition of the crimes of murder and manslaughter. Now, it may be said, if it is well settled, why disturb it? My answer to that is twofold. In the first place, I say that, although there is an unspoken understanding about the law by virtue of which it is administered in a very satisfactory way, yet the theory of the law, if you look to books, is very bad indeed. I say, in addition, that this sort of unwritten understanding which I am speaking of is one which sets every one to learn out of books. Another reason is that, although the state of things which I have described works very well and satisfactorily in quiet times and under favourable circumstances, I could very easily imagine a state of things arising in which it would work extremely ill, and in which circumstances of the most serious nature might be involved."

Passing over much that Mr. Stephen says with regard to homicide in general, we come to his answers as to the effect of insanity. He proposes to give an illustration of each of the subsections already mentioned.

"Subsection *a* is as follows:—'Homicide is not criminal if the person by whom it is committed is, at the time when he commits it, prevented by any disease affecting his mind from knowing the nature of the act done by him.' A is, by a disease affecting his brain, led to suppose that B is a wild animal, and shoots him accordingly; it is not denied that in this case insanity excuses him. Then comes subsection (*b*), 'from knowing that it is forbidden by law.' That is universally admitted; the narrowest interpretation of the judges' opinions in Macnaughten's case would show that; and it would produce this curious result, which, however, sufficiently explains what it means. Suppose a man was under an insane delu-

sion that an Act of Parliament had been passed by which murder had ceased to be criminal; then, if he goes and shoots anybody, according to that interpretation, he is freed from the consequences of his crime. Subsection (*c*) excuses a person prevented by disease affecting his mind 'from knowing that his act is morally wrong.' The word 'morally' is considered as enlarging the existing law. I cannot believe that it does, and I wish to put one or two illustrations upon the subject which are real illustrations. A man believed himself to be Jesus Christ, and also thought it was essential for the salvation of the world that he should be put to death, also that he must not kill himself, as it would be wicked; therefore, that he might be hanged and the world saved, he went and murdered some one else. Now, if you interpret the word 'wrong' as meaning not morally wrong, but forbidden by law, that man ought to have been hanged, for he not only knew that the act was forbidden by law, but he did it for this very reason. I can hardly argue with anybody who says that a man in such a monstrous state of mind as that ought to be hanged. Taking those as illustrations, you have two men before you—the one commits a murder because he is under an insane delusion that Parliament has passed an Act authorising him to do so, and he is not hanged, but confined in an asylum; the other commits a murder under the delusion above mentioned, and he is to be hanged because he knew what he did was forbidden by law. I do not think the public would ever agree to that. That leads one to ask, if it is admitted that the second man is not to be hanged, why is he not to be hanged? The answer is, because he is not in the state of mind which the law presumes in those whom it addresses and threatens. When you pass a law punishing a man for his crime, you are dealing with a being whom you presume to know that on a great many familiar grounds, quite independent of any mere fear of punishment, he ought not to commit crime; who knows, for instance, that if he does commit murder he causes extreme distress and great fear, and, it may be, ruin to his neighbour, and does a thing cruel and brutal in itself. That is the assumption on which all criminal law proceeds, that it is addressed to ordinary reasonable beings; but it seems to me that if a man by bodily disease is placed in such a position that he cannot feel that at all, and does not know the nature of his act and its consequences, he ought to be locked up in a lunatic asylum, and that exactly as much whether the nature of his delusion does or does not prevent him from remembering that the law has forbidden his act. Subsection (*d*) applies to a man prevented by disease affecting his mind 'from controlling his own conduct.' Now that, again, is a case which it is said ought not to form part of the law, and I have often heard judges say that it is a monstrous thing for anybody to believe in what are called irresistible impulses, and that the doctors who give evidence upon these trials constantly put forward as irresistible an impulse, which is not, in fact, resisted; and no doubt they do. I have had a great deal to do with cases of this kind. I had to try a case of that nature on the circuit last summer, and I have been

counsel in them, and there is no doubt that doctors do come forward and make statements which seem to be extremely foolish on the subject. But it is not because a man makes a foolish statement on that subject that the law is to put itself in a false position. It seems to me that if the fact is that there are diseases of such a nature that my arm may suddenly rise and plunge a knife into some person near me, just as mechanically as if I were in a convulsive fit, so to treat me as a murderer for doing it would be monstrous. And it also appears to me that the question whether or no such diseases do actually exist is a question of fact which the law ought not to prejudge in any way whatever; and if it be the case that there are such diseases, if that is proved by people who are skilful in these matters, I cannot see why that should not be left to the jury as well as any other fact, it being always pointed out to the jury by the presiding judge that it is one thing not to resist an impulse, and another thing to be under the prey of an irresistible impulse in consequence of disease."

Mr. Stephen then goes on to comment on the other part of this section, which says, that "if any person is proved to be labouring under any insane delusion at the time when he committed homicide, it shall be presumed, unless the contrary appears or is proved, that he did not possess the degree of knowledge or self-control hereinbefore specified." Of this he says he should like to give some explanation.

"Suppose a man is of opinion that his hand is made of glass, and suppose, without any assignable reason, he goes and kills somebody, there is no traceable or immediate connection between a man's thinking that his hand is made of glass and his killing a person, and you may certainly say with perfect truth 'Even if your hand is made of glass, or if you think so, that does not give you any right to kill people.' But then it is also true that where you have a specific definite delusion of that kind it shows that the mind itself is so deeply disordered in all kinds of ways, you cannot draw the inference that there was an intention to kill or do grievous bodily harm from the fact of killing, as you could in other cases. In ordinary cases, if you know merely that one person kills another, and know no more about it, you treat that as a great crime, and the reason is because you presume that everybody knows the natural consequences of his acts, and has the ordinary power of controlling them; but when you get a man under any definite delusion whatever, for aught you can tell, the workings of his mind may be such that the act which appears to you as murder appears to him in quite another light; and if you read books which give accounts of the workings of the minds of mad people you will find that directly you get a definite delusion set up, the process of the mind is vitiated as well as the mere result. The delusion runs through everything."

And Mr. Stephen goes on to illustrate this by the well-known case of the man who so loved windmills that he mur-

dered a child because he hoped to be locked up for it in some place where he might see windmills.

"My feeling is that when you find a person's mind in so strange a state that a positive delusion of any kind exists in it, the burden of proof ought to shift. You commonly presume a guilty intent from the act of killing, but where delusion is present the presumption does not arise. I do not say that a man under a delusion ought not under any circumstances to be held guilty of murder; on the contrary, I think in many cases it might, as this bill says, 'be proved or appear' that the delusion had nothing to do with the act, so that the ordinary presumption applies. Take such a case as this: A patient in a lunatic asylum labours under various delusions, and amongst other things, quite apart from his delusions, he has some grudge against one of the keepers, and in order to gratify it he kills him; I say that that man ought to be hanged just as if he were sane, and that because the crime is a sane crime, that is to say, because it 'would appear to be proved' from the ill blood between him and his victim, and that he did 'possess the degree of knowledge and self-control specified' in the bill with reference to the act of killing. In short, if a man was under a variety of delusions, and nothing else appeared except the delusions and the killing, then I would give him the benefit of the doubt; but if there appeared a bad motive, as well as the killing, I would say you must take the consequences just as a sane man; there is no difference between you and a sane man; that is the explanation of that section."

The next witness examined was Baron Bramwell, and of the Subsection *c* he says this:

"There is a matter here which I certainly confess I feel very strong upon; it is Section 24, Head *c*:—'Homicide is not criminal if the person by whom it is committed is, at the time when he committed it, prevented by any disease affecting his mind from knowing that it is morally wrong.' Now, I put this question to Mr. Stephen, or at least I sent this in to the Home Secretary: 'A crazy fellow knows it is forbidden by law to knock out my brains, but has a notion it is morally right, because I once tried a Fenian; is it to be no crime?' I think that is a mistake. He says 'I presume the "notion" arises from a disease affecting the 'crazy fellow's mind' (yes, certainly, I assume he is crazy), whereby he is prevented from knowing that it is morally wrong to kill a judge who tried a Fenian. If not, the section does not apply; but making that assumption I say distinctly no crime has been committed, because one of the conditions which the law assumes in dealing with men is that their moral feelings are on its side to the extent, at all events, of understanding that acts of atrocious wickedness are atrociously wicked, and that apart from law they ought not to do them; and this element is wanting in the case supposed.' He says further, 'I do not believe in the possibility of neglecting this consideration in practice, as the following case will show;' and then I will not trouble the com-

mittee with it unless they would like to hear it at full length, because I can state it concisely. It is the case of a man who supposed that if he could be put to death he would save the world, and then he murders somebody to get hanged in order to save the world. Mr. Stephen says, 'Would Baron Bramwell say that he ought to be hanged?' (I beg to say that is not the question. The question is, What rule you are to lay down; are you to lay down a rule that would exclude his being hanged?) 'Hardly, I think, but if not, why not? He not only knows what he is doing, and that it is forbidden by law, but the fact that it is so forbidden is the very motive which makes him do it. Clause 24 *c* exactly meets such a case. The man is under a monstrous delusion, which prevents him from attending to or judging of the moral characters of what he does, and ought no more to be punished than a dreamer is blamed for what he does in his dreams.' I have put in answer to that, 'I would not have a law to except this case, which would necessarily except others that ought not to be excepted.' The question is what rule you are to lay down. In the particular case he cites, probably you would let the man off being hanged, but that he ought to be held guilty of murder I have no doubt. Whether I would hang him is another matter. And when Mr. Stephen says 'one of the conditions which the law assumes in dealing with men is, that their moral feelings are on its side, to the extent, at all events, of understanding that acts of atrocious wickedness are atrociously wicked, and that, apart from law, they ought not to do them;' I deny that, to my mind, the law has anything to do with it, but it simply intends to terrify the man, to threaten him; it cares not what his views are on the subject, but says 'that is a thing you shall not do.' I do not quite understand the passage; and it would be very odd if it were true of moral and not of religious feelings, which religious feelings have continually driven people into the commission of crime. I have got this further note, that my answer is that what you want to do is to frighten people, to terrify them; and the way to try who ought to be punished, to my mind, is to try who ought to be threatened with punishment. Mr. Gurney may remember that I sent him a long disquisition of mine upon the subject, which I am not going to trouble the committee with; but if you can find out what man's mind is accessible to the influence of fear, you can find out the man you should punish, because those whom you threaten you ought to punish, that it may not be *brutum fulmen.* You ought to threaten every man sensible to the effect of a threat; and if this crazy fellow knows that what he thinks a virtuous and moral action is one which will cause him to be hung, I shall feel much more safety after having tried Fenians than I could otherwise feel. A similar reasoning seems to me to apply to Subsection (*d*) 'from controlling his conduct.' I think that is a great mistake. I have sent into the Home Office this note, 'I vehemently protest against *d*.' What is the meaning of a man being prevented controlling his conduct? When he is prevented it is because the preventing motives are strong enough. When he is not prevented it is because they are not strong enough. The effect of this would be to lessen

the preventing motives. A. wishes to commit a rape. Disease of mind weakens his power of acting on motives of honesty, religion, morality, goodness, &c., but the fear of the law added to those motives makes him able to resist. The proposal is to take away one of his good motives. At least this should be qualified by saying 'from controlling his conduct by the ordinary motives of mankind.' Now, I should like to mention to the Committee two things. One is that this illustration of a rape is not an imaginary case, for I have positively tried a man who I am certain had a mania on the subject. It occurred in Monmouthshire, where there are a number of narrow valleys parallel to each other, with a considerable hill or mountain between them, and the people had occasion to go from one valley to the other; and this man was a most moral and religious collier; was deacon in his congregation; earned larger wages than anybody in the neighbourhood, and had a first-rate character; but he had the infirmity of waiting on the top of one of these hills and knocking women down and treating them with atrocious violence. There were nineteen indictments against him. I tried him on five, and on all he was convicted; the others would not come forward. So this case of a rape is not an imaginary one. Then as to men controlling their actions, I tried a man named Dove, many years ago, for murdering his wife; he called a number of witnesses for the purpose of proving that he could not control his actions. There was one of them who, to prove the state of this man's mind, proved that he had shot a cat in the presence of his wife, or something of that sort; and this man gravely said that he believed it was an uncontrollable impulse. I put this question to him (I did not let him see the difficulty it would land him in; I got his mind away from the particular answer that he had given). 'Now, suppose a policeman had been present when he shot that cat, do you think he would have been restrained?' and he said 'Yes.' 'Well, then,' I said, 'according to your view, an uncontrollable impulse is an impulse acting upon a man when a policeman is not present.' It is obvious that what is called an uncontrollable impulse is one as to which the deterring or controlling motives are not strong enough; and this is a proposition in all cases to take away from a man in a state of mind in which he is more likely to do mischief than anything else a deterring motive."

Baron Bramwell goes on to remark that, although the present law lays down such a definition of madness, that nobody is ever really mad enough to be within it, yet it is a logical and good definition. Asked if he can express the state of the law as it is now, he replies:

"I really think I can. I have had to do it a great many times, and I ought to be able to do it now. When a man's state of mind is such that he does not know the nature and quality of the act he is doing, for instance, does not know that cutting a man's head off will kill him, like the man who cut off the head of another person in order to see how he looked when he woke; when this is his state

of mind, or when his state of mind is such that, although he may know what the result will be, he does not know that it is wrong, then he ought to be acquitted. So also if he were labouring under a delusion of such a character that if it were true he would be justified in the homicidal act, that is to say, if he supposes that the man he kills is attacking him, and that it is necessary to defend his life; in all those cases he would be entitled to be acquitted upon the ground of insanity. The common notion that a man may be acquitted merely because he is mad is erroneous."

Finally, he says that he should prefer leaving the law as it is upon the subject of responsibility and insanity.

Then comes Mr. Justice Blackburn, who is very sceptical indeed as to the advantage of what is commonly called codification:

"Very sceptical whether you can find it easy to reduce the law to a proposition; and if you do, I think it will be better not to lay down a rigid definition, because you are sure to leave something out which you ought to have brought in, or to bring in something which you ought to have left out. It is much better, it seems to me, to have the elasticity of the common law, and to apply the general principle to the particular circumstances."

With reference to the plea of insanity he says:

"The next thing that comes here is that homicide is not criminal in cases of insanity. To that I can only say that on the question what amounts to insanity that would prevent a person be punishable or not, I have read every definition which I ever could meet with, and never was satisfied with one of them, and have endeavoured in vain to make one satisfactory to myself; I verily believe it is not in human power to do it. You must take it that in every individual case you must look at the circumstances and do the best you can to say whether it was the disease of the mind, which was the cause of the crime, or the party's criminal will; but this I am clear about, whatever definition you give of insanity, it should apply to all crimes. I have tried men whose successful defence was that they committed arson when they were insane and not responsible. I have tried a woman who had cut the throat of a girl of fifteen; she was accused of wounding with intent to murder, and I thought she was properly found not guilty on the ground of insanity. With regard to the words in the 24th clause, 'from knowing that it is morally wrong,' or 'from controlling his own conduct,' these are the definitions that are put in, and these agree pretty nearly with what was said in Macnaghten's case, in their extra-judicial opinions, by the judges in the House of Lords; but we cannot fail to see that there are cases where the person is clearly not responsible, and yet knew right from wrong. I can give you an instance which shows what I held deliberately; it was in the case of that woman who was tried for wounding a girl with intent to murder. The facts were these: the woman had more than once been insane, the insanity being principally brought on by

suckling her child too long; that was the cause that had produced it before; she was living with her husband and had the charge of this girl, a girl of about fifteen, an impotent girl, who lay in bed all day; she was very kind to her and treated her very well. They were miserably poor, and very much owing to that she continued to nurse her boy till he was nearly two years old, and suddenly, when in this state, she one morning, about 11 o'clock, went to the child of fifteen, lying there in bed, and deliberately cut her throat; then she went toward her own child, a girl of five or six, of whom she was exceedingly fond, and the girl hearing a noise, looked up and said, 'What are you doing?' 'I have killed Olivia and am going to kill you,' was the answer. The child, fortunately, instead of screaming, threw her arms round her mother's neck and said, 'No, I know you would not hurt your darling little Mopsy!' The woman dropped the child, went down stairs to a neighbour's house, told her what she had done, that she had killed Olivia and was going to kill Mary, "but when the darling threw its arms round my neck I had not the heart to do it." She clearly knew right from wrong and knew the character of her act; for some little time after that she talked rationally enough, but before night she was sent to a lunatic asylum raving mad, and having recovered, she was brought to be tried before me at a subsequent assize. On the definition in Macnaghten's case, she did know right from wrong. She did know the quality of her act and was quite aware of what she had done; but I felt it impossible to say that she should be punished. If I had read the definition in Macnaghten's case, and said, 'Do you bring her within that?' the jury would have taken the bit in their own teeth and said 'Not guilty, on the ground of insanity.' I did not do that; I told them that there were exceptional cases, and on that the jury found her not guilty on the ground of insanity, and I think rightly. On this definition I think you would be obliged to say that woman was guilty.' Asked if there were not delusions, Mr. Justice Blackburn replied 'No, there was the subsequent evidence that before night she was so mad that they had to send her to an asylum; but before the crime, and some time afterwards, there was nothing whatever to show a delusion. But I fear a general rule of this sort, making it a question for the jury, whether the disease was the efficient cause of the act, would be leaving the thing at large; I have never been able to assign a definition satisfactory to my own mind, and will not pretend to do so. Now, I would further say that, as to what follows, Mr. Stephen defines it fairly enough; the two definitions, I believe, would include most cases of murder; and as to the rule put at the end as a canon of evidence, that people are to be presumed to intend and to know the natural and ordinary consequences of their acts, it is very good, but I do not see what business it has in a Bill of Homicide. Let us have the evidence kept separate."

Mr. Fitzjames Stephen is further examined, and he comments on the evidence of the two learned judges:

"As to Baron Bramwell's evidence on the subject of the principle

of the bill, I need say nothing, because Baron Bramwell approves. Mr. Justice Blackburn disapproves of it, upon the ground, which is very familiar to all who have taken much interest in discussions about codifying the law,—that an uncodified state of the law is better than a codified state, inasmuch as the law possesses a quality which he describes as elasticity, and which he regards as a good thing, but which I and some others should describe as uncertainty and vagueness, and regard as a bad thing. Baron Bramwell thinks that the law of England is such that insanity hardly ever, under any circumstances, excuses a man from crime; in fact, in one of his answers he goes so far as to say that he holds the definition of insanity to be logical and correct, but does not believe that anybody ever was mad enough to fall within it. Practically, that comes to the same thing as saying that madness makes no difference as to responsibility. Mr. Justice Blackburn says, on the other hand, that the section drawn in the bill pretty nearly represents the existing law as it is, and if it errs, it errs in defect because it does not take in certain cases which ought to be taken in. He adds that in a particular case which he had to try, thinking that the existing law was, I suppose, in a very elastic condition, he took upon himself to tell the jury that there were exceptional cases which came under no rule, and that they ought to acquit the woman who was on her trial, on the ground of insanity, although no authority could be found for it, and although Baron Bramwell, on equal authority, considers that the woman, under such circumstances, ought not to be acquitted, as it was perfectly certain she was by law guilty."

Commenting further on the evidence of the two judges, he says:

"I think any one reading Baron Bramwell's evidence carefully will see that the result of it is simply this, that insanity ought to be no excuse at all for crime; and with every respect for Baron Bramwell, that appears to me a monstrous proposition. There is just one bit of his evidence which I will refer to specially, in order to say that it marks a fundamental difference between him and me. He reads a passage from a letter which I wrote to him in which I say that 'one of the conditions which the law assumes in dealing with men is, that their moral feelings are on its side to the extent at all events of understanding that acts of atrocious wickedness are atrociously wicked, and that apart from law they ought not to do them,' to which he says 'I deny that to my mind the law has anything to do with it; but it simply intends to terrify the man, to threaten him; it cares not what his views are on the subject, but says that is a thing you shall not do.' Now, that is a fundamental difference between him and me, which there is no use in discussing. I say that you cannot treat people in that way, and that you ought not to try to do it; that practically if you are to threaten and terrify men by law, it must be by a law which they feel is a terror to evil doers. You must have their consciences on your side if the law is to be of any real moral weight whatever; and I say that if the law were so constructed that people who were obviously thoroughly mad, and

did not know the moral quality of their actions, were to be punished under it just as if they did know the moral quality of their actions, it would destroy all respect for the law; and I do not believe that it would add in the slightest degree to the safety of mankind. However, that is a very wide question and I do not go into it.

"I will only call attention to his concluding remark, which, I think, justifies my observations: 'Although the present law lays down such a definition of madness that nobody is ever really mad enough to be within it, yet it is a logical and good definition.' Well, if you define a thing in such a way as to give a general definition which includes no particular case, it is news to me that it is a good and logical definition. I will not pursue that further, beyond saying that I can leave Mr. Justice Blackburn to answer Baron Bramwell. Mr. Justice Blackburn takes exactly the opposite view, and thinks that the section, as I view it, does not go half far enough; and that, in fact, the judge ought to be under no rule at all, but ought to have a right to tell the jury that a given case is exceptional, and that he thinks this and that person ought to be acquitted."

Lord Chief Justice Cockburn did not give evidence before the committee, his judicial duties rendering his personal attendance impossible, but he communicated in writing his views upon the subject of the bill, to which he objects as—

"A partial and imperfect attempt at codification, the law relating to homicide forming only part of the law relating to offences against the person, while this, again, forms only a part of the criminal law. A still more serious objection is, that by any enactment specially applicable to a subordinate branch the latter may be brought into anomalous disaccord with the rest of the department of law to which it belongs. Of this we have a very striking instance in the provision of this bill with regard to insanity. The bill introduces a new principle, which it makes applicable to homicide alone. As the law as expounded by the judges in the House of Lords now stands, it is only when mental disease produces incapacity to distinguish between right and wrong, that immunity from the penal consequences of crime is admitted. The present bill introduces a new element, the absence of the power of self-control.

"I concur most cordially in the proposed alteration of the law, having been always strongly of opinion that, as the pathology of insanity abundantly establishes, there are forms of mental disease in which, though the patient is quite aware he is about to do wrong, the will becomes overpowered by the force of irresistible impulse; the power of self-control, when destroyed or suspended by mental disease becomes, I think, an essential element of responsibility. But while it would be desirable to establish the law on this basis, it certainly is not desirable so to establish it as to homicide, while in the closely cognate case of the offence, doing grievous bodily harm, a different law would obtain."

Further on he says:

"I have already expressed my concurrence in the proposed provisions as to the effect of insanity, except so far as the proposed legislation is partial, as limited to the case of homicide. But there is one general provision on this subject to which I must strenuously object; it is that 'if a person is proved to have been labouring under any insane delusion at the time when he committed homicide, it shall be presumed, unless the contrary appears or is proved, that he did not possess the degree of knowledge or self-control hereinbefore specified.' The pathology of insanity shows that the mind may be subject to delusions which do not in any degree affect the moral sense or the will as regards the power of self-control. The mere existence of mental delusion ought not to affect the decision as to the power of self-control, unless the nature of the delusion be such as legitimately would lead to the inference that the power of self-control was wanting. The question is one which should be decided by all the circumstances, independently of any presumption one way or the other.

"I have further to observe that the provisions of Section 24 are confined entirely to insanity as arising from disease. No provision is made for congenital malorganization; in other words, for idiocy; nothing is said as to the responsibility of children. If it be said that this is to be sought in the general law as to the capacity to commit crime, it is obvious that the same thing applies to insanity, with which the bill professes to deal; another instance of the inconvenience of partial legislation as here proposed."

Dr. Russell Reynolds, in his paper on "The Scientific Value of the Legal Tests of Insanity," says:

"My final object is to propose some mode of remedying the evils which at present exist, and that which occurs to me to promise the best results is to inaugurate a conference between the legal and medical professions upon this matter, with the purpose of taking such steps as may be thought most conducive to the end in view."

This conference, if we are rightly informed, never took place. But in the evidence which we have just quoted we have a valuable mass of legal opinion from men who are at the present time in the front rank of their profession, higher, we may venture to say, than any whose time and position would have allowed of their entering upon such a conference as that suggested. Having thus procured the legal opinions, let us examine them briefly and compare them with those of medical writers so far as we can acquaint ourselves with them.

Dr. Reynolds suggests various points to be considered at such a conference. We may briefly notice some of these:

1. The arrival at a better definition of insanity generally.

2. A revision of the tests of insanity: (*a*) that based upon the existence of delusion; (*b*) that turning upon the knowledge of right and wrong and of the consequences of actions.

3. An examination, in all its bearings, of the doctrines of partial insanity and its responsibilities.

7. A determination of the mode to be adopted in dealing with cases, both civil and criminal, when insanity is alleged as a plea of innocence or as a bar to disposing power.

8. The possibility and desirability of doing away with the present mode of investigation in a court of law, viz. by the calling of skilled witnesses on different sides.

Although at the present time the same tests of responsibility are applicable in all cases where insanity is set up as a defence, yet greater attention has always been directed to the plea in cases of homicide than in comparatively minor offences, and this for several reasons: first, homicide is the gravest offence known to our law, the only one for which capital punishment is awarded; secondly, homicidal patients are a class apart, differing in many respects from those who commit the lesser offences; thirdly, where the offence is less grave and the punishment is imprisonment in some form or other, the criminal, if insane, can always be relegated to an asylum, but mistakes cannot be amended after the executioner has done his work.

The discussion, in fact, resolves itself into the question whether lunatics ought or ought not to be hanged. Beyond a doubt some people, possessed by the abject fear of lunatics, which unfortunately is still so prevalent, think that the latter deserve to be hanged rather more than the sane. Dr. Maudsley tells us that "an English judge, still on the bench, said that he was not sure whether it was not more necessary to hang an insane person than a sane person. The opinion, barbarous as it seems, is evidently based on the belief that it is necessary in the interests of society to deter insane persons from doing murder, and that the execution of them will act as a warning to other madmen, and so deter them, if not from going mad, at any rate from doing murder when they are mad." This is precisely Baron Bramwell's opinion as given before the Homicide Law Committee. We can only say to Baron Bramwell and such as think with him that no really insane person will be deterred from committing murder by the fear of being hanged, any more than he will be prompted to commit murder by a knowledge that he, being a lunatic, will not be hanged. Physicians who pass their lives among the insane know that insanity is a disease which renders them unlike other people, and uninfluenced by the fears and motives of sane people. Patients are so sane in many respects that it is the constant difficulty of those having charge of them to impress upon nurses and attendants the fact that they are not to be held

responsible for the thousand acts of wickedness, mischief, and folly they commit, which try the temper of all and provoke retaliation from such as forget that were they like other people they would not be in an asylum. But to punish one would never deter another for a single moment.

Dr. Maudsley says that the punishment of death should never be inflicted on an insane person, but that it is another question whether such a one should not be otherwise punished under certain circumstances. But, after all, the modes of punishing such people are but few: what more terrible punishment can there be than lifelong imprisonment whether in a jail or in a criminal asylum?

Although Dr. Reynolds speaks of the tests of insanity, yet in dealing with the plea of insanity in criminal cases we have to examine the test not of insanity but of responsibility, the knowledge of right and wrong, and the nature and quality of the act. Insane persons are or are not responsible according to this test. Such persons come under the category of the "partially insane," whether the insanity be termed impulsive, moral, or intellectual. Under the head of partial insanity also may be placed transitory attacks of mania with or without epileptic seizures.

We cannot help thinking that the opinion so firmly held hitherto by the majority of lawyers, that the partially insane are responsible for their actions, is due to the doctrine formerly enunciated by legal luminaries, that a lunatic is one who hath sometimes his understanding and sometimes not, *gaudet aliquando lucidis intervallis.* The doctrine of lucid intervals is one which is purely legal. For ourselves we have endeavoured assiduously to discover them, and except in the somewhat rare cases of recurrent mania we have failed to do so. As late as 1812, on the trial of Bellingham, Lord Mansfield said "that in the species of madness called lunacy, where persons are subject to temporary paroxysms, in which they are guilty of extravagance, such persons committing crimes when they are not affected by the malady, would be to all intents and purposes amenable to justice." We hear little now of lucid intervals, but the partially insane are still amenable to justice and to capital punishment, and the partially insane form the great mass of the insane. It is a mistaken though a very common notion that a madman is a raving maniac or a demented idiot. The majority are neither the one nor the other, but, insane on certain subjects, they know perfectly well what is going on around them, and can converse rationally often for a long time, and on all but a few topics. They may or may not indulge in violence or extravagance of conduct, but inasmuch as their

eccentric or violent acts are the outcome of their insane ideas and feelings, it would be as rational to hold them responsible for the latter as for the former, and this as yet nobody has proposed to do.

Partial insanity is divided by Dr. Maudsley into—1, affective insanity, the subdivisions of which are (*a*) impulsive and (*b*) moral insanity; and—2, partial intellectual (or ideational) insanity. In studying these classes, however, we must bear in mind that they are divisions of insanity rather than of the insane, and that the same patient may at one time deserve to be called morally insane, while later he may present delusions and well-marked intellectual insanity. It has been laid down by various lawyers, notably by Sir John Nicoll in Dew *v.* Clarke, that delusion must be present to constitute legal insanity. The reason why criminals suffering from impulsive and moral insanity have been held either to be not insane, or at any rate responsible, is because these are often the early stages of an insanity which at a later period stands unquestioned and unmistakable. Such people are prone to commit homicide at this early period because their malady is in an acute stage, and culminates, often to the great and immediate relief of the patient, in some frightful deed.

Impulsive acts of homicide are committed by lunatics of various kinds, and this must be borne in mind lest we endeavour to bring into one class patients who differ widely, and so by inaccuracy diminish the value of scientific description. There may be cases for which no better name can be found, but the majority may certainly be brought under heads which indicate a disease and not one symptom common to many diseases. It may be melancholia, and this is the form more frequently than is imagined—a feeling that something terrible is impending which leads the sufferer to put an end either to himself or to others to save them from the worse evil that threatens. It may be "voices" upbraiding and accusing—a form of persecution which exercises a most overwhelming influence on the patient. In place of an epileptic attack, during the unconscious state which sometimes takes the place of a convulsion, or preceding or following one, a homicidal impulse of a most furious nature may urge the individual to violence, or it may be the first symptom of a recurring mania, as in the case of the woman mentioned by Mr. Justice Blackburn. It may be the senseless wanton impulse of an imbecile or idiot; who kills a man or a child as he would torture an animal or stone a frog. And above all it is to be remembered that such impulsive acts may be symptoms of that great pathological division of insanity which shows itself in so many and various ways,

hereditary insanity. How many there are of whom it is difficult to say whether they are mad or not. Inheriting in greater or less degree an insane temperament they may pass through life without committing criminal acts or being placed in an asylum, yet they may be the pest and torment of all around them. If they do commit criminal acts it is difficult to say whether they are responsible or not. If the act is a minor offence they are probably sent to prison; if it be homicide it is a matter chiefly of accident whether they are hanged or not. In the latter case we may be able to detect no definite symptoms of insanity prior to the act, which may be the first marked outburst of the disease, but either before or after, if opportunity is afforded, it will for the most part be possible to notice peculiarity, eccentricity, or insane modes of accounting for what has been done, which, coupled with the hereditary taint, may leave no doubt as to the source of the homicidal deed. It is the whole history of the case which is to afford the evidence of the insanity, not the apparent resemblance of the act to other acts which are said to be those of impulsive insanity. Although Dr. Maudsley gives the latter as one of the divisions of partial insanity, and homicidal insanity as a subdivision, yet in a former chapter of his book he says:

> "Instead of seizing upon a prominent mental symptom such as an impulse to suicide, homicide, theft, incendiarism, which may be met with in a particular case, and thereupon making such pathological entities as suicidal mania, homicidal mania, kleptomania, and pyromania, which have no existence as distinct diseases, the aim of the inquirer should be to observe carefully all the bodily and mental features, and trace patiently in them the evolution of the cause. Given a case of insanity in which homicidal impulse is displayed, he will observe with what other symptoms the impulse is associated, will thereupon refer the case to the natural group to which it belongs, and set forth its relations to its cause; so he will present an accurate picture of a real disease, instead of concealing inadequate observation under a pretentious name, and offering the semblance of knowledge by the creation of what can be described only as a morbid metaphysical entity."[1]

When judges have given over talking about the knowledge of right and wrong, and the law conforms in some degree to the provisions proposed to be enacted by the Homicide Bill from which we have quoted, it will be for the medical witness to say whether the accused could or could not control himself when he committed the impulsive act of violence.

Dr. Bucknill, one of the Lord Chancellor's visitors in lunacy, has well remarked:

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[1] Op. cit., p. 81.

"Responsibility depends upon power, not upon knowledge, still less upon feeling. A man is responsible to do that which he can do, not that which he feels or knows it right to do. If a man is reduced under thraldom to passion by disease of the brain, he loses moral freedom and responsibility, although his knowledge of right and wrong may remain intact." [1]

Another form of partial insanity is the so-called moral insanity, and here the same argument holds good. Moral insanity is said to exist in patients whose intelligence is unimpaired, but whose feelings, affections, propensities, temper, habits, and conduct are changed and perverted, so that the individual is totally altered, and feels and acts in a way he would never have dreamed of acting when he was in his sane state. Yet he has no definite delusion or hallucination. Hence, his insanity is said to be not intellectual, but moral. Now, as in the so-called impulsive insanity, various insane persons may at different times present these features, and yet pathologically the disorder each is suffering from may vary much. And we must endeavour to make out the history and pathology of each, to lay our finger on the disease itself, not merely on one symptom, and thus to arrive at an opinion as to the responsibility or non-responsibility of the offender. The task is easier here, for a crime is seldom the first thing noticed; there is a change in the individual; the quiet man has become hilarious, the gay is dull and heavy; the prudent man squanders his money in foolish purchases or debauchery; the staid and reserved lady thinks all are in love with her, and persecutes men to whom she is almost a stranger. But such is the beginning of a vast amount of insanity marked by acute symptoms and delusions of all kinds. It will be found that moral insanity is often but a stage, to be followed by other and graver outbreaks. The patient, may, however, remain in this stage, and not go beyond it. He may recover from it, or may live for years unable to control his acts, doing foolish and wrong things if unrestrained, bitter and hostile in feeling to friends and relations, changed in every essential, but having no delusions save and except the one great delusion that it is meet and right that he should conduct himself in this outrageous manner.

We shall have to look about for some cause of this change, and shall often find it in that inheritance which is so constantly the origin of mental disease. Another fertile cause is drink. Men and women saturate their nervous systems with alcohol, and it affects them differently. Some contract *delirium tremens*, others pass into a state of dementia with loss of memory

[1] 'Unsoundness of Mind in relation to Criminal Acts.'

or partial paralysis, but a certain portion are affected to the degree above mentioned, and remain so at any rate for a time, recovering therefrom or passing into a more confirmed state. The same condition may also be due to epileptic or apoplectic attacks, and is not unfrequently the commencement of senile insanity.

A change which may truly be called moral or emotional is noticeable in many patients who afterwards display very varied symptoms of insanity. This is the simple depression or melancholy which, according to some observers, is to be found at the beginning of every insane case. Slight as this may seem to the bystander, there is no condition in which suicide, and homicide also, is more frequently committed. Such cases may be read in almost every newspaper. And as it is the commencement, so it is often the termination of an acute insanity. Acute excitement, even hilarious and boisterous violence, may give way to depression, and because the patient has become rational and can talk on many points, it is supposed that he is well, and release from restraint is followed by some shocking act. It is ever to be borne in mind both by those who have to treat, and those who have to give evidence concerning persons of unsound mind, that changed feelings are as important indications of mental disorder as any that can be found, and that even if a patient has given up all delusions and confessed their folly, he is not to be thought well if his conduct and expressions indicate a change of feeling to those he formerly loved and esteemed.

Where a great change has come over a man, insanity may be reasonably expected; but there is a class of patients who may be said to be morally insane, and are from childhood peculiar and unlike other people, either wholly depraved, or compounds of goodness and badness intermingled in a strange and insane fashion. They may be deficient in intellect and incapable of education, or sharp in some pursuits, clever but wayward, and totally devoid of any moral sense or ideas of right, duty, or honour. It is often very difficult to prove these patients insane, yet were one of them to commit homicide the presumption would certainly be that he was not responsible lik ordinary people. Such an one was the Alton murderer, quoted by Dr. Maudsley as an instance of homicidal impulse. This youth had been always eccentric, and at one time it had been necessary to watch him lest he should commit suicide.

"He was plainly an instinctive criminal, if he were criminal. The impulsive character of the crime, the quiet and determined ferocity of it, the savage mutilation, his equanimity immediately afterwards, and his complete indifference to his fate—all these indicated an

insane organization, ill-attempered, a discord in nature, which, had it not issued as it did, would, sooner or later, have ended in suicide or in unequivocal insanity."

It is curious that these very characteristics are to Dr. Hammond the reason why such a person should be hanged. "Some of the insane are such monsters of depravity that they should be slain, upon the same principle that we slay wild and ferocious beasts." Speaking of the same homicide he says:

"The evidence at the time showed that a near relative of his father was in confinement, suffering from homicidal mania, and that his father had also been insane. It was likewise proved by many witnesses that the prisoner was unlike other people; that he was subject to attacks of melancholy, during which he would weep without evident cause; that his conduct had been capricious, and that it had been necessary to watch him, for fear he would commit suicide. There is more than a reasonable probability that this wretch was insane, but the jury disregarded them; a verdict of guilty was rendered and he was executed."[1]

From this we may deduce the proposition that the greater the insanity and moral obliquity the greater is the reason why such persons should be hanged, and there is no doubt that many are of this opinion. These are the people who would come under the clause *c* of the Homicide Bill as "prevented by disease affecting the mind from knowing that the act is morally wrong."

Another division of partial insanity is that characterised by hallucinations or delusions, which has been termed *intellectual* or ideational insanity. Here we may meet with every degree from the patient, or monomaniac as he is called, who presents but one delusion to the man whose delusions are numberless, and his insanity general and unquestioned. With regard to delusions the theory of the law as it at present stands is that a criminal is responsible or not according to the character of the delusion. For example, if under the influence of a delusion, he supposes another man to be in the act of attempting to take his life, and he kills that man, as he supposes, in self-defence, he would be exempt from punishment. If his delusion was that the deceased had inflicted a serious injury on his character and fortune, and he killed him in revenge for such supposed injury, he would be liable to punishment.[2] This the Homicide Bill proposes to abolish, and to enact that if a person is proved to have been labouring under any insane delusion at the time of the committal of the crime, it shall be presumed that he did not possess the requisite degree of knowledge or self-control

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[1] Op. cit., p. 55.
[2] Maudsley, op. cit., p. 97.

unless the contrary appears or is proved. The Lord Chief Justice, however, demurs to this, and says the mere existence of mental delusion ought not to affect the decision as to the power of self-control, unless the nature of the delusion be such as legitimately would lead to the inference that the power of self-control was wanting, and that the question should be decided by all the circumstances independently of any presumption one way or the other. The Lord Chief Justice's views as regards delusions were enunciated at some length in the celebrated cause Banks *v.* Goodfellow. On that occasion he said:

"No doubt, where the fact that the testator has been subject to any insane delusion is established, a will should be regarded with great distrust, and every presumption should in the first instance be made against it. Where insane delusion has once been shown to have existed, it may be difficult to say whether the mental disorder may not possibly have extended beyond the particular form or instance in which it has manifested itself. It may be equally difficult to say how far the delusion may not have influenced the testator in the particular disposal of his property. And the presumption against a will made under such circumstances becomes additionally strong where the will is, to use the term of the civilians, an inofficious one, that is to say, one in which natural affection and the claims of near relationship have been disregarded."

And he quotes Sir William Wynne, who, using the older legal phraseology of "lucid intervals," says:

"I think the strongest and best proof that can arise as to a lucid interval is that which arises from the act itself; that I look upon as the thing to be first examined, and if it can be proved and established that it is a rational act rationally done, the whole case is proved."

The Lord Chief Justice, speaking of a will, tells us that where a delusion is proved the presumption should be against it, but in the case of homicide there must be no presumption one way or the other. Yet homicide cannot be looked upon as a rational act rationally done. Dr. Maudsley lays down three considerations as the foundation of the medical doctrine by which monomania, that is, a single delusion, is held to exclude criminality.

"*a.* A delusion may be concealed, wherefore it may be overlooked, although it has actually affected the conduct. *b.* It is impossible to follow the working of an unsound mind, and to discriminate between a healthy and a morbid action thereof, it being beyond dispute that an act which a looker-on cannot discover to have any manner of connection with the delusion may still be the insanely logical outcome of it. *c.* It is impossible to isolate an insane delusion, and thus to prevent the infection of its morbid nature from spreading; it

being certain that the whole disorder in monomania is not restricted to one delusive idea, that the rest of the mind is in a more or less marked state of moral or affective alienation—in a state in which insane impulses to acts of violence are likely to occur."

He also remarks that lawyers, in the importance which they assign to delusion as a mark of insanity, vastly overrate its value.

"Not half the insane acts of a person labouring under general mania are really the offspring of his delusions; they represent the overflow of morbid energy, are often aimless and motiveless, so far as we can judge—the mere convulsive expressions of disordered nerve-centres."

This is perfectly true, but at the same time it must be remembered that delusions denote a somewhat advanced and confirmed insanity. The altered feelings of the earlier stage are converted into the insane ideas we call delusions, and the monomaniac who has but one is more of a madman than the morally insane man who is only altered in feelings and conduct; he who has one delusion to-day may have two to-morrow, and ten the next day. We should be very sorry to depend on the absence of others in the mind of a patient who appears to have one, or to count upon his insane acts as though he would do nothing insane except in connection with his one idea.

Baron Alderson (Reg. *v.* Stokes) said, "Who enabled them to dive into the human heart and see the real motive that prompted the commission of such deeds?" This was urged by him against an alleged lunatic whose crime was said to be motiveless. The argument is a sound one, but it may be adduced with equal force in the case of a crime which is thought to be, or not to be connected with a particular delusion.

Among the points which Dr. Reynolds wishes considered at a conference between the legal and medical profession are the possibility and desirability of doing away with the present mode of investigation in a court of law, viz. by the calling of skilled witnesses on different sides.

Although the lamentable exhibitions which medical men make in the witness-box are due in a considerable degree to their own shortcomings and ignorance of the subjects on which they testify, yet we must attribute the blame of such exhibitions mainly to the system of interrogating witnesses as now in vogue. In almost every other country of the civilised world a homicide of whom insanity is alleged is examined by a commission of physicians learned in such matters, who, after due inquiry, prepare a carefully written report as to the sanity or insanity of

the accused. Contrast this with our procedure. A murder is committed by some poor man; it may be far from cities in some country village. At the next assizes he is put on his trial. His friends, if they are poor, are unable even to fee counsel, much less to pay for experts to examine the prisoner and give evidence on his behalf. Some junior barrister is perhaps requested to defend him, and with no knowledge whatever of his previous history or that of his relatives he proceeds to do so. He may, perchance, set up the plea of insanity, but he has no witnesses and very little knowledge of the subject. The man is convicted, and after the trial, though a new trial is unknown to our system of jurisprudence, he is virtually tried over again in the Home Secretary's office, and is not unfrequently acquitted on the ground of insanity. Things may be somewhat better when public prosecutors are appointed; but at present, when counsel only care to win, when the most scientific witness may be cross-examined as if he were a fraudulent bankrupt, and every effort made to render his evidence unintelligible to the jury or unworthy of evidence, it certainly "cannot be denied," to use the words of Mr. Fitzjames Stephen, "that physicians will have to submit to the mortification of seeing, not only the jury, but the bar and bench itself, receive with scornful incredulity or self-satisfied ignorance evidence which ought to be received with respect and attention." Judges are very prone to tell juries that they are not to listen to "medical theories," and writers in the press insist that medical witnesses must bring demonstrable proof of that which they allege. But demonstration is not applicable to this any more than it is to other medical subjects. We cannot demonstrate typhoid fever in water or milk, yet the Legislature has been constrained by "medical theories" to pass Acts of Parliament in accordance with them. To submit theories of insanity to twelve British jurymen is as useful as it would be to ask their opinion concerning typhoid or cholera, and yet this is what has to be done constantly at the present time. All medical writers agree that, in estimating the responsibility of an alleged lunatic, it is necessary to examine into the history of the whole of his past life and that of his relatives, and to form an opinion based on the whole of the case. But if a witness proceeds to read a report drawn up on such materials he is told that it is not evidence; he must confine himself to what he actually knows and has seen himself; in short, the reports which are read as drawn up for the courts of America, France, or Germany, would be refused in our own, and the most important points fail to be elicited, because they are matters of scientific opinion which judges call "medical theory." And so

it comes to pass that, as Dr. Maudsley says, in criminal trials where insanity is alleged the issue might as well be decided by tossing up a shilling as by the grave procedure of a trial in court.

VIII.—Naval and Military Reports.[1]

In our last notice of these official reports of the two public services, in the number of this Review for October, 1873, we considered them mainly as affording instructive illustrations of geographical and climatological nosology. On this occasion we propose to view them in another and not less interesting aspect, namely, their bearing on our knowledge of the ætiology of certain diseases, a branch of medical inquiry on which the experience of the officers of the army and navy is capable of contributing most valuable evidence. The disease we have selected for present remark is Typhoid or Enteric fever, and this we do, both on account of the great practical importance of the subject, and also because of the existing diversity of opinion in the profession on various topics relating to it. The difference of views held by men of authority respecting the cause or causes concerned in the origination of this form of fever, and the uncertainty that still prevails as to the *degree* and *extent* of the morbific influence of impurities in drinking-water, and also as to the *nature* of such impurities, in its production, are only two of the several problems that still await definitive solution, or anything like it. Looking at the specially favorable opportunities possessed by the medical officers of both services for watching the initial history of disease among their patients, and for ascertaining all the antecedent and accompanying circumstances of its development, we may reasonably expect to obtain from them more exact and trustworthy evidence on these matters than can generally be acquired by practitioners in civil life. Let us now see what is to be gathered from the latest experience of our army and naval brethren on the difficult subject before us.

In 1871, among the troops in the United Kingdom (the strength of the force averaged 99,300), there were registered

[1] 1. *Statistical Report of the Health of the Navy for the year* 1871. 8vo, pp. 401; with Appendix, pp. 216. 1873.

2. *Statistical Report of the Health of the Navy for the year* 1872. 8vo, pp. 386; with Appendix, pp. 126. 1874.

3 *Army Medical Department Reports for the year* 1871. With Appendix. 8vo, pp. 453. 1873.

4. *Army Medical Department Reports for the year* 1872. With Appendix. 8vo, pp. 557. 1874.

80 cases of enteric fever, and of these 22 proved fatal, a large proportion certainly. Out of the entire number, 28 occurred at Aldershott, chiefly in the months of July and August, and 7 of these were fatal.

All that we are told respecting the origination of the fever is that "the outbreak at Aldershott was believed to depend on impure water and a defective state of the surface drainage."

The outbreak at Pembroke Dock hut-barracks in the preceding year was fortunately made the subject of a minute special inquiry by Dr. Massy, then the head of the sanitary department of the service, which well deserves notice. Between January 15th and March 11th there were 21 attacks and 4 deaths among the garrison, which seems to have averaged about 870 souls.

"On most careful inquiry of the three first cases (each of which happened in separate and non-adjacent huts) no extraneous origin could be discovered; the patients had all been several months at the station. The cases then occurred scattered, following no order of succession, either as regards occurrence in the same hut or in huts in proximity. In seven of the instances, only one case occurred in each hut; in five instances, two cases occurred in each hut; and in one instance, three cases. The eleventh case occurred in a new fort about two miles from Pembroke Dock, occupied by a small detachment as a working party. No other case happened there among the military, although cases occurred in the vicinity. * * * The last case was admitted on March 11th; the man had been sent on detachment from Pembroke Dock to Penally on the 2nd, and was attacked with fever on the 9th; before leaving Pembroke he had lived in No. 30 hut, where no previous cases had occurred. There was free and constant communication between the north and south camps, and yet all the cases, with the exception of two in children, occurred in the former division. As already stated, Dr. Massy could not satisfy himself as to the origin of the fever, and he failed to trace it to any single and specific cause. Although the water from one of the wells in the camp was suspected of having possibly become tainted with the contents of an adjoining drain from a latrine, this was not satisfactorily made out; and the Army Sanitary Committee of the War Office, after personal examination of all the circumstances of the case, came to the conclusion that the cause of the outbreak was traceable to 'the large latrine at the north-east angle of the camp, mainly through the pollution of the atmosphere, which, at the time, was carried by the prevailing air currents over and through the affected huts; and, in all probability, partly to impure surface-drainage leaking into the water-tank supplying most of the huts through defects in the drainage, since removed.' * * * 'Water from all the wells has been examined, with the object of ascertaining the present condition of the water as an element in the general sanitary state of the camp. They all yield nitric acid and organic matter, not

in any great quantity, but still sufficient to indicate the existence of oxidized organic compounds in the subsoil.' "

Cases of fever had occurred in the town of Pembroke and its neighbourhood, but the evidence of the extent, Dr. Massy says, was very unsatisfactory. Only a single case occurred among the men of the Royal Dockyard, as we learn from the navy report for 1870.

During 1872, among the troops in the United Kingdom, the number of recorded cases of enteric fever, and of deaths resulting therefrom, was almost exactly the same as in the previous twelve months. Nothing whatever is stated respecting the supposed origin of any of the cases.

In the garrison of Gibraltar there occurred in 1871 seven deaths (the number of attacks is not given) from this fever, respecting the source of which we only learn that "the medical officer in charge states that no local cause of this disease could be detected."

Although 14 deaths occurred during that year from "continued fevers" in the garrison of Malta, no specific mention is made of enteric fever as the cause in any of the cases, an omission to be much regretted.

In 1872 there were 11 cases, 3 fatal, among the troops in Gibraltar. "The disease is believed to have been contracted in the town." At Malta, there were no fewer than 29 cases, 10 fatal. "The cases, most of which occurred after the period of extreme heat, are attributed to the usual causes. The rooms in the Floriana Barracks are close and offensive in the morning, a fæcal smell being perceptible on the south-east side." No mention is made of the water supply, or of any other possible cause.

The marked exemption from fatal fevers among the troops in Canada is almost of uniform occurrence, in the statistical records of the health of the army.

Of 5 deaths from enteric fever among the military stationed at Bermuda in 1871, "no local cause of the disease could be discovered." In several former years, the disease had been more extensively fatal among both the military and civil population there. Respecting the outbreak in the 15th Regiment in 1869, the surgeon states that "he found it impossible to lay his finger on any specific cause for the prevalence of this fever." That year's report contained an extremely valuable paper "On the Continued Enteric Fever of Bermuda," which bears so appositely on our present subject that we are tempted to make a few extracts from it. The author, Dr. Don, of the Royal Engineers, had been resident for two years in the colony.

"During the hot months cases of enteric fever have occurred pretty simultaneously all over the island in isolated places, both among the civil and the military population. It may safely be affirmed that the origin of such cases could not be consistently explained on a theory of specific or merely local causation. I have known cases in localities where it was impossible to lay the finger on sanitary defects other than those universally existing in places where no disease had appeared.

"Various unhygienic conditions have, of course, with more or less show of probability been blamed in a manner indicating a lurking belief in a purely pythogenic origin rather than implying acquiescence in a specific causation. But in all such search for a causation the logic of facts seems to show that the exceptions to are quite as numerous as the pythogenic rule sought to be established. For instance, in accordance with the usually received opinions regarding the *media* through which enteric febrile diseases become self-propagating, tank-water has been at once suspected and blamed. Granting, however, that in some instances, among the lower stratum of the civil population, suspicion of fæcal water and contamination has amounted almost to certainty, the theory by itself completely breaks down if carried to Prospect Camp, where the said form of fever has been rather prevalent than otherwise. There the rainfall is immediately conducted in closed pipes from the roofs of the huts into large iron and stone cisterns, sewage contamination of which is impossible."

After alluding to the similar ununiform traceable connection between the occurrence of cases of the fever with the "cesspit system," the writer remarks:

"In the filthy purlieus of St. George's, any amount of tank and soil contamination is probably possible and conceivable; but the appearance of marked enteric disease in the newly formed camp at Port Royal in 1868, pitched on fresh soil and far removed from town infection, as well as in other equally isolated localities, could hardly, as in St. George's, be explained by any purely fæcal theory.

"Further, it must be borne in mind that both tank and soil pollution continues the same during the cool but mild winter as in the summer months; but the fever is nevertheless probably wholly in abeyance; fæcal contamination is now no worse, but probably greatly less, in and about habitations than in former times, when the enteric form of fever was (according to intelligent Bermudians) practically unknown."[1]

During 1872 only two cases, neither fatal, occurred at

[1] Whether this form of fever has really and truly become developed in Bermuda only of recent years, as is believed by several of the resident medical men, or whether this appearance be merely due to more correct diagnosis of the disease, all agree that it is vastly more prevalent now than it used to be a few years ago in St. George's and other places. The same remark probably holds good in respect of most of the military stations and towns in the West Indies, and elsewhere in tropical and subtropical regions.

Bermuda. To what extent the disease affects our troops in the West Indies it is not possible yet to determine, as it has not yet been uniformly discriminated among the group of "continued fevers" in the medical registers of the army. In 1871 no deaths at all are recorded from this cause; but in the previous year 3 fatal cases of continued fever occurred in the barracks in Trinidad out of a total of 71 attacks, and there is reason to suspect that the fever was of a typhoid character, as it had been extremely prevalent and fatal in the adjacent town of Port of Spain in the preceding year, when yellow fever also raged about the same time. This association of these two forms of grave pyrexial disease is a point in their history which deserves careful inquiry. The frequency of typhoid fever in Port of Spain was first clearly pointed out by Dr. Stone in two valuable papers in this Journal for 1868 and 1869.

In 1872-3 cases (1 fatal)—1 at Barbadoes, 1 at Trinidad, and 1 at Jamaica—occurred throughout the West India command.

Passing now from the West to the East Indies, the reader is struck with the marked *apparently* much greater frequency of enteric fever of recent years among our troops in India than formerly. The explanation is doubtless due to *subjective* rather than to *objective* causes—to change in diagnosis and nomenclature of the disease much more than to any positive increase in its actual prevalence.[1] In the Bengal Presidency 48 fatal cases, in that of Madras 15, and in that of Bombay 13, are registered in 1871 as due to this cause; but nothing is said in the body of the report as to the probable origination of the fever anywhere.

The appendix, however, contains a paper "On the Causes of Enteric Fever at Bangalore" in the two preceding years, by Deputy Inspector Dr. Barclay. In his opinion the exciting cause was, "for the most part," the use of impure water from wells, in a ravine which was notoriously foul, and was little better than a wide open latrine. He adds:

"That the *contagium*[2] is not in any extent air-borne I am unable

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[1] As bearing on this point, it may be noticed that among the troops (numbering 698) stationed in Japan during the first seven and a half months of 1871, "there was a great prevalence of enteric fever, sixty cases having been admitted, of whom six died. The disease broke out in June, and continued till the date of their withdrawal, August 7. It was not at the time epidemic among the civil population."

[2] The use in recent years of this term to designate the presumed *materies morbi* or "morbific cause" of such zymotic diseases as enteric fever and epidemic cholera is surely most unwise. Not only has it no warrant either in classical or in old medical Latinity, but it serves to mislead by suggesting the presence of an essential property or attribute of the hypothetical something, when that is, to say

to say, the ravine and villages being directly to windward of the barracks whenever the wind is southward, as it often is during the monsoon; but I do not think much evil can be produced in this way, as the greatest prevalence of the disease is not at the commencement of the monsoon, when the surface must be most foul, but towards its termination."

The Sanitary Commissioner of the Madras Government did not agree with Dr. Barclay's views as to the probable causation of the fever. The impurity of the water supply he believed to be without any effect in the production of the disease. He attributed it to exhalations from an abominably filthy bazaar close to the barracks, and where he had no doubt that "the specific poison which produces enteric fever exists, as it does in all the other bazaars at the station."

The number of deaths from enteric fever among the troops in India was in 1872 somewhat higher than in the previous year. Respecting the probable cause or causes of the disease in the different presidencies we learn as follows:—In Bengal "no facts of interest bearing on the question as to the invariable transmission of the 'contagium' from case to case, or the occasional production *de novo* as a result of putrescence of the specific poison which causes the disease, are brought forward in any of the reports." In Madras Dr. Massy states that, "on carefully considering all the circumstances connected with Cannanore, it is hard to say whether the prevalence of enteric fever there of late years may be attributed to local insanitary conditions, or to the influence of tropical climate acting upon an unusually large number of unacclimated constitutions. Possibly it may have resulted from the operation of both causes." The water supplied to the barracks "is reported by the analyst to be very pure and remarkably free from organic and mineral impurities." All that we are told respecting the history of the forty-two cases, twenty-five fatal, among the European troops in the Bombay Presidency is that "only a fifth of the admissions occurred during the first six months of the year; that there were no admissions during the first two months of the rainy season; but that the period of the greatest prevalence of the diseases was towards the end of that season; and that only four of the deaths occurred in men above thirty years of age."

In the recently published 'Vital Statistics of the Bengal Presidency,' Calcutta, 1874, Dr. Bryden has made some highly interesting and instructive remarks on the subject now before

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the very least, only of occasional and not of invariable existence. The word "miasm" is altogether preferable; it is of classical origin, clearly indicates what it is meant to express, and involves no risk of erroneous conjecture.

us; and as all his observations rest on the most conscientious examination of very ample and authentic evidence from an immense field of inquiry, they may well claim the respectful attention of the profession at large, whether they be accepted in full or not.

The most frequent cause of death in recent years among young European recruits—lads of twenty years old and under—in the Bengal Presidency has been shown to be "continued fever," which is, in almost every instance, says Dr. Bryden, true typhoid or enteric fever. Three fourths of the deaths from this cause occur in the hot months. Young men up to twenty-four years of age are almost equally liable to suffer, and cases continue to be pretty numerous between that age and thirty. Various theories have been advanced to account for the outbreaks of the disease in different parts of India. Importation and the insanitary condition of the locality where the case or cases occur, and the state of the water-supply, have been urged as the main or sole causes of the disease. But this is a narrow view of this difficult question. Numerous well-observed facts clearly demonstrate that it is at times liable to become developed independently of any or of all together of these agencies. That "the sudden change to a tropical climate predisposes to the spontaneous development of typhoid in young and weakly constitutioned lads" cannot, he thinks, be reasonably doubted. Its occasionally synchronous manifestation in different localities over extensive regions seems to indicate the operation of, at times, a wide-spread general seasonal influence.

"In the last ten days of August, 1872, almost every station over several enormous areas began to return cases of enteric fever. Secunderabad and Poonah, in the Deccan; Nusseerabad and Mhow, in the north of the Bombay Presidency; Kurrachee and Hyderabad, in Scinde; and Murat and Umballa, in Northern India; all returned in the same week, and after an interval of months, during which no typhoid had happened, fatal cases of the disease."

Dr. Bryden is by no means disposed to question the possibility, or even the likelihood, of a morbific element being generated in a typhoid patient capable of transmitting the disease to another person. All that he takes exception to is the doctrine that, without the introduction of typhoid excretory matter in some shape into the systems of the young recruits landed in India, they are safe from the development of the fever.

"My observation tends to teach that, while it may be perfectly true that typhoid is in many instances so propagated, the specific lesion and its attendant fever are capable of development without the

application to the system of a poison elaborated elsewhere; and that the ætiology of typhoid is not comprehended within the limits to which, of late years, the tendency has been to seek to confine it."

We quote one other short extract:

"The fact that enteric fever may occur among young soldiers during the voyage out does not necessarily imply that typhoid was taken on board at the port of embarkation; and all statements regarding the importation of typhoid should be reviewed with due regard to the fact that the sudden change to a tropical climate predisposes to the spontaneous development of typhoid in young and weakly constituted lads."

Murchison has recently observed, Dr. Bryden adds, that "we must study anew the ætiology of enteric fevers by the light thrown on the subject by the ascertained history of typhoid in the British army in India."[1]

We shall now pass on to the sister service, and see what the experience of the navy teaches us on the subject. In 1871 the number of cases registered in the returns as enteric fever was 64, of which the great majority occurred, as usual, in the Home and in the Mediterranean fleets. In the former there were 28 cases in twelve different ships. Five terminated fatally. In the latter there were 18 cases in six different ships, and of these also 5 were fatal. No explanation respecting the origin of the disease in the home fleet is given beyond the general rather unsatisfactory one that it was contracted on shore. No distinct reference is made to the use of impure or tainted drinking-water as its immediate exciting cause. At Malta, to the polluted state of whose harbour in the dockyard creek most of the cases in the Mediterranean fleet are traced, mention never occurs of the water supply there being even so much as suspected; it is to the foul effluvia from the creek that allusion is invariably made. On two or three occasions we are merely told that "the cause of the attack could not be ascertained." The fever appears never to have exhibited any contagious properties on board the ships that suffered. In 1872 the number of recorded cases in the total force afloat was 48, "of which 20 were on the Home station, 12 on the Mediterranean, 1 on the North American and West India station, 1 on the West Coast of Africa and Cape of Good Hope station, 1 on the China station, and 13 in the irregular force." Again, the ætiological evidence is very much of the same character as in the preceding year. The 20 cases, six fatal, in the home fleet occurred in seven different ships. Most of them were in boys or youths. One was in "a seaman who was sent to Haslar with epididy-

[1] 'The London Medical Record' for March, 1873.

mitis. While in hospital febrile symptoms set in, which soon assumed the specific character of enteric fever. During its course hæmorrhage occurred from the nose, mouth, and the bowels, and the case ran a steadily downward course. The eruption peculiar to this fever was very scantily present on the abdomen."

As before, most of the cases (12 in number, 3 deaths) in the Mediterranean fleet "were attributed to infection contracted at Malta" in consequence of the loathsome condition of the dockyard creek.

As specimens of the history of outbreaks of the fever in our ships of war on other stations besides those already mentioned, we shall select one instance from the Pacific squadron in 1871 and one from the irregular force in 1872.

In the former, which consisted of ten ships, having an average total complement of 2240 on board, there occurred 6 cases of enteric fever during the year, 1 in the *Scylla* and 5 in the *Zealous*.

"The case in the *Scylla* was of great severity, although the man ultimately recovered. Its origin could not be traced, the man not having been out of the ship for several months except for about two hours on the Sunday previous to his seizure, when he went to visit a messmate in the Naval Hospital at Esquimault. He was unwell at the time, and there was no infectious diseases in the hospital when he visited it."

The omission of the dates here is to be regretted, more especially as some of the cases in the *Zealous* appear to have been sent to the same hospital on shore. For any progress to be made in ætiological inquiries, exactitude in chronological details is indispensable.

"The first case (among the crew, 540 in number) occurred when on our voyage from the Sandwich Islands to San Francisco in May. The young man had been for many days on the sick list before he gave any indication of serious mischief existing; the pulse was good, the skin only was drier, and the temperature somewhat above the normal standard, with a tendency to dryness of the tongue, which increased after many days, and it became slightly brown in the centre. Finally, all these symptoms became greatly aggravated, abdominal tenderness set in, accompanied with loud gurgling on pressure over the iliac fossæ, and frequent copious liquid bloody motions. The lungs also became engorged and congested, resulting in typhoid pneumonia, with constant low muttering delirium and incessant restlessness." From this illness he, however, slowly but steadily recovered on board, and he was landed at Vancouver to recruit his health.

In the early part of July four others of the crew (one a patient with bad sore throat in the sick bay) sickened with the early symptoms of the fever; but although "in all these there was tenderness on pressure being made over the iliac fossæ, chiefly over the right, accompanied with gurgling," and in two of the cases "there was much hæmorrhage from the bowels with bloody sputa," all the patients recovered and returned to duty.

Dr. Mackay may well express surprise that "no information is given as to any probable source whence the first case of this formidable form of fever originated," and the more so as the medical officer is a man of large experience and high in the service.

The next instance we shall quote is that on board the steamer troop ship *Orontes*, with a crew of 210. The narrative is extremely interesting, and reflects much credit on Staff-Surgeon H. Loney.

"From time to time during the year we have been visited by this disease in a severe and fatal form, isolated cases having occurred on board at different periods, sometimes with long intervals between the times of their appearance, two cases seldom happening simultaneously, and no two cases appearing to be consequent upon or connected one with the other. The first case was placed on the sick list on the 12th of February, when the ship was in the Red Sea on her passage to China, a month out from England and eleven days from Malta. The disease appeared in a boy, æt. 18, who had been on leave shortly before coming on board at Portsmouth, but had not been on shore since that time. He looked emaciated and badly nourished; stated that he had been previously ill for some days (how many he could not tell). Complained of headache, soreness of surface, pains in the limbs, debility, and loss of appetite. The feet and ankles were painful and swollen, with patches of ecchymosis over each malleolus. The abdomen tumid and tender on pressure, with distinct gurgling in the right iliac fossa. Tongue red, glazed, deeply and strangely chapped in all directions. Bowels irregular, at first constipated, afterwards loose; skin hot, pulse quick. The progress of the disease was at first slow and insidious, the patient lying in about the same condition day after day until the 24th of February, when he became markedly duller and more stupid, and towards evening slightly delirious, and the temperature of the body rose to a greater and more constant heat. On the 26th profuse diarrhœa set in, the abdomen became more swollen and tympanitic, the urine diminished in quantity. On the 27th, in addition to these symptoms, pain and crepitation were developed in the right chest, and the breathing became embarrassed; 28th, more prostrate. Stools passed from him. Urine retained, necessitating the use of the catheter. Increased weakness, dyspnœa, and stupor. 29th, became unconscious, had lost the power of deglutition. Died the same evening. * * * Careful

examination had failed in detecting any decided characteristic eruption on the body in this case; the rose rash, if present at all, being represented by a few stray papulæ on the abdomen and inside of the thighs. There was no appearance of hæmorrhage from the bowels at any time; the urine did not contain albumen."

We omit the particulars of the treatment.

"No other case followed the above, the ship continuing to be free from the disease for nearly three months—in fact, until she was at Trincomalee on her homeward voyage, when it happened in the person of one of the stewards employed in the saloon. He complained on the 3rd of May of much debility and of hepatic derangement, tenderness of abdomen, and irregularity of the bowels, with hot skin and quick pulse. He was landed at Trincomalee at the temporary hospital, far better accommodation, while the ship proceeded to Negapatam, to land a Sepoy regiment she had on board; and he was re-embarked on the 12th of May, just previously to the ship's sailing for England."

The case was reported by the medical officer of the hospital as one of hepatitis, with enlargement of the spleen.

"When brought on board the patient was very weak, and prostrate; all doubts about the nature of his disease were set aside by its taking on decidedly the form of enteric fever, the symptoms increasing in urgency as his strength decreased, following a similar course as in the previous case, until he also succumbed from pneumonic complication on the 24th of May; the ship was then in the gulf of Aden. In this case, also, the rose rash was absent; but a copious crop of sudamina appeared over the breast and abdomen during the latter part of his illness."

The next case on board occurred when the ship, after having passed through the Red Sea and the Suez Canal, had reached Malta, the subject this time being a marine who had been invalided home from Her Majesty's ship *Princess Charlotte*, at Hong Kong, for chronic dysentery for which he had been under treatment during the voyage home.

"In this case the new disease was masked, in some measure, by the old; but the appearance on the abdomen and flanks of relays of the rose-coloured spots, the increased and persistent heat of the skin, the sudden prostration of strength, and very rapid pulse, helped to point out the advent of the fresh disease. This man was conveyed on shore to Malta hospital on the 13th, and he died somewhat suddenly on the 15th of June. A post-mortem examination there showed that there had been no error as to the nature of the disease."

It so happened that, about the same time, another case of the fever occurred on board in one of the stokers, æt. 21, of a weakly constitution, who had been two or three times on the voyage relieved from his duty on account of debility, caused by

working at the fires during great heat, and who on June 13th applied (he had been ill, he said, for some time before he came to the sick bay) in consequence of diarrhœa accompanied by abdominal pain. Next day the diarrhœa continued, and there was greater abdominal tenderness with iliac gurgling; the skin was hot, and the pulse quick and weak, and there was an indistinct eruption on the body. On the following day he was sent to the hospital on shore, and there he died fifteen days after. Neither of the two last-mentioned patients had been on shore at Malta, before they had been conveyed sick to the hospital.

"Diarrhœa and bowel complaints prevailed among the ship's company while we were at Malta and on the passage home; but it was not until the ship had arrived at Portsmouth, and had landed all her troops, passengers, &c., that the next case of enteric fever was put on the sick list. On the 5th of July the warrant officer's cook, a coloured man, native of Brazil, complained of headache, vertigo, and pain in the chest and abdomen. He said he had been ill two days previously. Bowels constipated, tongue white, and skin hot. On the 6th he was sent to bed, the symptoms having become aggravated; the skin was pungently and persistently hot, and the tongue drier and browner. He was therefore sent at once to Haslar Hospital, where after a long illness he recovered."

Within ten days of the occurrence of this case another of the crew, a stoker, æt. 21, was attacked with similar symptoms when the ship was at sea in the Irish Channel. He was landed at Kingstown. The subsequent illness was severe, but he recovered, and rejoined the ship on the 12th of August in a weak condition, so that he could not return to duty for some time. A week or two subsequently to this man's attack, another stoker, about the same age, fell sick much in the same way as his comrade, and he was sent to Haslar where he died.

The concluding observations of the report are extremely interesting:

"Thus it will be seen that there were seven cases of enteric fever occurring on board the ship in the period of twelve months. Of these, five belonged to the ship's company, and two were supernumeraries taking a passage. Two cases were treated entirely aboard, the remainder at first aboard and afterwards at hospital and sick quarters. The disease was fatal in five cases out of the seven—a large proportion. The first case was put on the sick list on the 12th of February, and the last on the 20th of July. The men attacked were all young, the oldest being twenty-five years of age. No conclusion was arrived at as to the origin or cause of the appearance of this fever on board. The same disease was prevalent at the different places called at on the voyage, and also at the home ports, being more than usually rife at Portsmouth. But it is possible that it may have originated in the ship herself, as, from the nature of their employment, troop ships and emigrant ships must be more

liable to be visited by pythogenic disease than vessels of other descriptions, from the fact of their containing so many people crowded together in a small space, using a limited number of latrines and water-closets, and, as in our case, exposed for a long time to a very high temperature. Every effort was made to keep all the places on board likely to generate disease cleaned and disinfected. The latrines, bilges, decks, &c., were scrubbed, and whitewashed, or painted. Carbolic acid and other disinfectants were freely employed about the ship, and personal cleanliness enforced among the crew and the soldiers.

"Against the theory of the disease having arisen in the ship herself is the fact that no case of the fever showed itself among the soldiers, women, children, or other passengers on board the ship, either on the voyage out or home. The seven attacked by the fever were not all messed together, nor did they even sleep on the same deck, so that nothing points to a local cause. The water drank on board was condensed or distilled from sea-water, and the tanks containing it were kept sweet and clean.

'There was no evidence of contagion, the patients attacked by the fever having been kept apart by themselves. Carbolic acid or Condy's fluid was used freely about the hospital and mixed with the excreta of the patients in the bed-pans. Three of the sufferers were stokers, young men who had undergone hard work at the fires at a great heat for a long time, and who had, in consequence, become debilitated, and rendered therefore, probably, more susceptible to noxious influences. The disease increased in frequency after the ship's arrival at home in comparatively cool weather, and ceased somewhat suddenly without apparent change in the condition of the crew or alteration of the circumstances surrounding them."

It will render the preceding narrative more complete if we briefly allude to the report from Haslar Hospital (to which two of the cases were sent) on the history of the enteric fever in that establishment during the year. Twenty-three fresh cases were admitted, which, with two remaining, made up the number of twenty-five under treatment.

"These cases were admitted from different localities and ships; the major number, however, came from the Indian troop ships; but the vessels were not in fault, for the men who contracted this fever had been on leave, and had been living in places infected with the disease. In the majority of the cases the symptoms declared themselves insidiously; many were admitted for simple feverish symptoms and only after some days in hospital did the characteristic signs of enteric fever make their appearance. Among these twenty-five cases eight deaths occurred; the greatest mortality obtained among those admitted during the Michaelmas quarter. In the worst cases hæmorrhage from the stomach and bowels was witnessed, and, on examination after death, Peyer's patches were found diseased, and ulceration of the intestines had been the immediate cause of death."

Before we quit the evidence in these official reports on the subject we have been considering, it will be profitable to glance at Dr. Parkes' report on hygiene for 1873, in the appendix to the last 'Army Blue Book,' wherein he has sought to balance the relative merits of the two ætiological theories of enteric fever at present in vogue, viz. that of the *exclusive contagionists* (or transmissionists), who hold that its development and propagation are always due to the transmission, somehow, of its morbific poison from one patient to another, and that of the *in part contagionists*, who, while admitting the occasional agency of this cause in its production, maintain that the disease also frequently originates spontaneously, *i. e.* independently of the pre-existence of its poison derived from antecedent cases. Dr. W. Budd is considered the representative in this country of the former, and Dr. Murchison of the latter doctrine. Dr. Parkes himself leans, doubtless, to Budd-ism, but he candidly admits that "there are cases in which we cannot fairly assume such an explanation (any traceable or presumable connection with a preceding case of the disease), and till the evidence is much more complete Dr. Murchison can hardly be dislodged from his position." Dr. John Harley, who has discussed this point with great acumen in the article on typhoid fever in Reynolds's 'System,' confirms, we observe, Murchison's opinion.

The summary given by Dr. Parkes of the recent elaborate discussion at Munich on this important problem is extremely interesting.

"It will be seen from this short account of this able debate that the question of typhoid is in Munich pretty much in the same state as in England. The view that the disease arises from a specific agent; that this agent is transportable is admitted; that it can arise spontaneously (*i. e.* not derived from previous cases) is held by some; that it is truly contagious by others; that it spreads through the air by almost all; that it spreads through water and food by many, but not by all; that it is influenced by and perhaps generated in the ground, and is greatly affected by the movements of the ground-water, is greatly but not universally admitted."

We have now only to express our hope that our brethren in the army and navy will have their attention specially drawn to the problem which at present engages so much professional interest, and that the next annual reports of both services may contain instructive material that will aid in its solution.

The very short remaining space at our command cannot be more profitably occupied than in briefly noticing the course of the geographical diffusion of the epidemic Dengue which appears to have prevailed throughout the East Indian and China seas in 1871 and 1872, as it is a point of much epidemio-

logical interest. In the early part of the former year it broke out at Zanzibar and the neighbourhood, not long after the subsidence of epidemic cholera in that country during the previous autumn. Whether the region between Zanzibar and Aden became affected we cannot tell. The disease reached the latter place about the end of June, and quickly spread among the whole population, European and native; very few escaped. It seems to have lasted till about the end of August. While it was so universal on the land, the ships of war in the harbour remained nearly exempt, although they had all the time free and constant communication with the shore. Only five cases occurred among the crews of the *Dryad* and the *Nimble*, about the end of July and beginning of August. The disease showed no tendency to spread on board ship, though all the medical men considered it to be undoubtedly contagious on shore, and believed that it had been imported by arrivals from Zanzibar.

The next place we hear of being visited is Bombay; the date of the visitation is not given. From Bombay it spread inland to various military stations. Calcutta was attacked some time afterwards about November; in a few weeks nearly the whole immense population of the city seemed to have come under its influence. Whole households were often attacked at once; and yet, on the other hand, in numerous instances a single member only of a family suffered while the others escaped, or a single individual escaped while all around were seized. The epidemic is stated to have been at its height in March, 1872. It seems to have spread over the whole presidency, as we read that in the army "it prevailed throughout the command during the three last quarters of the year, and caused 4026 admissions, but no deaths. In almost every report the extreme contagiousness of the disease is dwelt on."

In the Madras Presidency it appeared at Caunanore, on the Malabar coast, about the beginning of 1872, but did not reach Madras until April, nor Trichinopoly, intermediate between that city and Cape Comorin, until September. Our data are probably much too imperfect to warrant entire reliance on the above statements. Whether the epidemic visited Ceylon we are not informed. It is said to have been imported into Burmah in the month of April, and it seems to have prevailed there during the next three or four months.

From the 'Navy Report for 1872' we learn that 36 cases occurred in the East India squadron, during the year, in four different ships, the majority of the attacks being in the *Daphne* in the month of June, while she was stationed at Rangoon. In none of the instances does the disease, notwithstanding its accepted great contagiousness, appear to have spread much

among any of the crews. The same thing was the case with the only ship in the Chinese squadron that was affected, the *Elk*, while she lay at Amoy, where the epidemic prevailed from August to the end of October, during which time nearly ninety-five per cent. of the entire population of the city had, it is believed, suffered. "The disease travelled from Amoy to the adjacent towns and villages. It was also conveyed by sailing vessels from Amoy to the island of Formosa, where the town of Tai Wan Foo was affected much in the same proportion as Amoy had been." The medical officer of the *Elk* adds that in his opinion "the disease must have come to Amoy with Coolie emigrants returning from Singapore, where I have heard the disease had been raging." We strongly suspect that dengue, like influenza (which it manifestly resembles in many of its epidemiological features), can spread from place to place and region to region quite independently of human inter-communication, although this may favour and accelerate its transmission. It may be taken for granted that our knowledge of the real extent of the geographical area over which the epidemic of 1871-72 was diffused is extremely imperfect, and little more than merely partial and fragmentary. Incomplete, however, as it is, it is obviously very instructive, and we therefore earnestly trust that our professional brethren in the two public services will in future avail themselves, more and more, of the favorable opportunities so often at their command of enlarging the amount of exact information as to the origin, development, and course of spreading migratory diseases in every part of the world.

IX.—The Vital Statistics of Insanity.[1]

The English and Scottish Lunacy Reports give a fair representation of the extent to which lunacy prevails in the population, of the public and private provision made for lunatics, and of the condition of those unfortunate beings themselves. The result of a general review of them comes to this—that, on one hand, we have cause for congratulation in the vast efforts made to afford lunatics those special provisions for their protection, safety, and general well-being, which the humane impulses and doctrines of recent times have adjudged necessary, and, further, in the general success that has attended

[1] 1. *The Twenty-eighth Report of the Commissioners in Lunacy to the Lord Chancellor.* Ordered by the House of Commons to be printed, July 13th, 1874.

2. *Sixteenth Annual Report of the General Board of Commissioners in Lunacy for Scotland.* Edinburgh, 1874.

those efforts; but that, on the other, we have much to deplore in the failure of our machinery to keep under the growth of lunacy by effecting the cure of the insane. And failure in this latter respect, it must be confessed, is not compensated by success in the other, unless we take a lesson of despair, and rest content with housing and tending the insane as the sole attainable end of our efforts to deal with them.

But a policy of inaction with respect to curative treatment cannot be acquiesced in. The incurability of the insane was a persuasion of the popular mind in past times, and may still be so of the few; but it has been one of the many struggles on the part of the medical profession in the interests of humanity to dispel so sad and paralysing a notion, and to show that insanity is a curable disease. And this doctrine of its curability must still be cherished, however unpromising may be the prospect of an apparently interminable accumulation of incurables in asylums throughout the land, and of an increasing ratio of the insane to the population, facts which the present reports, like their predecessors, too distinctly proclaim.

The questions naturally arise—How are we to reconcile those facts with the doctrine of the curability of insanity? And if this doctrine be true, where lie the causes adverse to its demonstration? If we turn to the reports before us we can get no sufficient answer; and yet, in our humble opinion, they are questions of paramount importance, and well worthy the serious attention of the Lunacy Boards. We do not wish to imply that they are never entertained in some shape or other by the Commissioners in Lunacy in each division of the kingdom, for this would be to ignore many useful observations and inquiries made by them, and particularly by the officials on the Scottish Board; bnt we are fain to say these problems are only nibbled at, not exhaustively discussed, as they might be by those so advantageously placed as are the Commissioners to do it.

To turn, however, to the business in hand, viz. to place before our readers an analysis of some of the statistical facts contained in the latest issued lunacy blue-books respecting the lunacy of Great Britain, and to accompany it with certain comments suggested by their consideration. We shall follow with a few remarks on the general facts, the recommendations and reflections advanced by the Commissioners with regard to the insane, their condition and management. As a matter of course we can in this review deal only with a selection of topics illustrating the vital statistics of the insane; to discuss the whole subject would require a treatise.

As before intimated, the statistics published exhibit an increasing number of lunatics, year by year, under official

cognizance. The English Commissioners report a total of 62,027, being an increase on the sum total of the preceding year of 1731, of whom 269 were maintained in private asylums and private houses at their own charge, and 1462 were paupers chargeable on the rates. The Scotch report shows a total of 7982, and an increase on that of the year preceding of 133; modest figures compared with those obtaining in England, where the increase is thirteen times that which has taken place in Scotland, and, consequently, after making allowance for the difference of population in the two countries, an increase of alarming proportion.

But, alas! even these huge figures do not represent the total number of disordered and imbecile minds in the kingdom. For, as the English report points out, the last census returns showed an excess in England of nearly 11,000 such disordered intellects over and above the number officially known at the same date to the Lunacy Board, and the reporters still calculate on the existence of 10,000 not on the registers. These waifs and strays from official cognizance were in Scotland reckoned a few years since at 2000, but the Commissioners entertain the conviction they are at the present day less numerous, "from the increasing tendency to accept parochial relief." In the two divisions of the kingdom, moreover, some few hundreds must be added, representing those found lunatic by inquisition and not officially visited by the Commissioners.

We come consequently to the melancholy conclusion that some 82,000 of our fellow-countrymen, in England alone, are mentally deranged or idiotic, incapable of civic duties, debarred from civic privileges, unproductive units in the population, an incubus and tax upon their friends and neighbours. Reflection on the fact is calculated to create, if not alarm, certainly misgivings and uneasiness, particularly when it is coupled with the further fact that, in round numbers, each year adds 2000 more lunatics to the number of the one preceding, and that the ratio of insane to the population steadily grows year by year; so that, if the same proportionate increment continues until 1879 the ratio per 1000 to the population will in England, among registered lunatics alone, have advanced from 1·86 to 2·86. Indeed, we apprehend that, on taking into account the number of unregistered insane, the present ratio to population reaches 3 in every 1000. Our neighbours north of the Tweed have the consolation that, as a people, they are somewhat less mad than we on the south, having only about 2·4 lunatics in 1000 of their number, and a ratio of increase less rapid.

The Scottish Commissioners "leave it open to doubt whether the increase of officially known lunatics is due to a greater pre-

valence of lunacy among the people, or simply to a growing disposition to concentrate lunatics in asylums" (p. iv). Reference to the statistical tables do at least in a very large measure solve this doubt, so far as England is concerned, by showing a progressive increase of admissions into asylums (Table, p. 8), a non-progressive—nay, a retrogressive—ratio of recoveries, and a stationary ratio of deaths. In other words, the insane are admitted into asylums in a constantly increasing ratio, whilst recoveries and deaths together remain at a fixed ratio; and, consequently, there must be an increasing residuum of incurables, and an ever-augmenting accumulation of insane under care and treatment. If we accept these facts, as vouched for by the official tables put forward by the English Commissioners, and translate them into common language, what else is it than that there is a "greater prevalence of lunacy among the people," year by year? The only doubt that can be raised is, whether this augmenting prevalence follows a higher ratio than that of the actual increase of population; but, under all points of view, we must recognise it as a fact that there is a rapidly increasing accumulation of insane folk. The only set-off against this fact is that, according to the statistical tables, there is a greater proportion of late years of asylum patients removed unrecovered; a circumstance, be it observed, calculated to keep slightly under the rate of accumulation in asylums, but not the relative proportion of insane in the population.

Examination of the Scottish Lunacy Report will exhibit results differing only in degree, and on the whole more encouraging.

To the Scottish Commissioners great credit is due for their persevering endeavours to elucidate the causes which affect the prevalence of insanity in a given population and the growth of admissions into asylums. They demonstrate by several examples a direct relation between pauperism and pauper-lunacy, subject, however, to the influence of various extrinsic circumstances, and they point out certain conditions which materially affect recourse to asylums. In illustration we may quote their remarks on the discrepancies observable in the proportion of pauper lunacy in different counties, taking the example adduced from the returns of lunatics in Perthshire and Renfrewshire. In the former county, with a population of 127,768, there are 389 pauper lunatics; whilst in the latter, with 216,947 inhabitants, there are only 249.

"This result [they observe] does not depend upon the more frequent occurrence of lunacy in Perthshire, for Table XV shows that many more pauper lunatics are annually intimated in Renfrewshire than in Perthshire. Various causes probably contribute to produce

the higher proportion of persistent pauper lunatics in the former county, such as the smaller ability of the poorer classes in Perthshire to maintain their insane relatives without parochial relief, the more acute and less enduring form of lunacy in Renfrewshire, and the more limited amount of the fatal forms of insanity among a population chiefly rural, like that of Perthshire, than among one chiefly urban, like that of Renfrewshire." (p. vi.)

Moreover, the movement of the patients in these "two counties is so different, whether as regards admissions, discharges, or deaths, that it is very clear the results shown by the tables must be greatly influenced by the operation of causes extending far beyond the asylum walls. One cause which must materially affect the results lies in the far more active movement which takes place among the general paupers of Renfrewshire, compared with that which occurs among the general paupers of Perthshire." Thus, it happened that the number of poor in Perthshire relieved during the year exceeded the number on the roll on 14th May by 691, whereas in Renfrewshire the difference in excess was 2331. Facts such as these show that neither the lunacy nor the pauperism of a district can be correctly estimated by the number appearing on the registers on a given day; and accordingly it becomes necessary, in instituting comparisons between the results of different asylums, to take into account those manifold external influences which largely modify both the number and the condition of "the patients when received. That the results must be influenced by the circumstances in which the patients are placed when in the asylum is, of course, obvious."

In the matter of recourse to asylums great diversity exists, as above mentioned, in different counties. One cause of this is unquestionably due to facility of access to the asylum. The operation of this cause has been frequently insisted upon both by the English and Scottish Boards. It has doubtless been brought about in some instances by the economical considerations of parochial authorities, who have decided to save the cost of transfer to a distant asylum. But besides this and other supposable motives, it must be admitted that there is something in familiarising the public with the character and organization of asylums which must favour recourse to them, for even at the present day prejudices and suspicions haunt the minds of many, including even those who would resent being spoken of as the less instructed classes, respecting the condition and treatment of the insane in those public institutions.

There are, moreover, other influences at work affecting the proportion of lunatics sent to asylums in different counties, among which is the character of the population as regards

wealth, occupation, density, and geographical distribution. These points are discussed in the Scotch report. For instance, it is pointed out that the highest proportion of lunatics remitted to asylums is met with in Argyleshire and Perthshire, "which are both back-going counties as far as population is concerned." Yet it might have been expected that, in the case of a poor and sparse population, a higher proportion of patients would be placed in private dwellings, and a lower proportion in asylums, than in populous counties like Edinburgh and Renfrew. Indeed, in Orkney and Shetland, where such features of the population are especially marked, the proportion of patients placed in asylums is the lowest among the Scottish counties. The difference remarked between the two first-named counties —Argyleshire and Perthshire—and the two insular ones, with respect to resort to asylums, must be found especially in the geographical features of the latter rendering access so much the more difficult, since a general similarity obtains among all these counties in sparseness of population and in poverty of means, as likewise in the unfitness of the cottages generally of the peasantry to make home management practicable.

Further, as the Commissioners observe—

"The movement among pauper lunatics is regulated by social and industrial conditions of very variable force; and it may be accepted as an axiom that the placing of lunatics on the poor roll, and their removal from the poor roll, go on with considerable more activity among an urban, busy, and restless population, than among one which is rural and settled in its habits. Unless this fact be kept in mind in forming estimates of the amount of lunacy occurring in different districts, the conclusions arrived at cannot fail to prove erroneous and misleading. The procedure under which patients are placed in asylums and removed from them will also materially affect the results. Thus, in Renfrewshire, where the asylums are entirely under parochial management, ephemeral cases of insanity, many of them the result of drinking bouts, are without hesitation placed in asylums by inspectors of the poor, in the knowledge that they are not thereby removed from parochial superintendence, and that no extraneous or opposing influence will interfere to prevent their discharge at the earliest moment this is considered practicable." (p. xli.)

Nor must we in the opinion of the same authorities lose sight of another circumstance influencing variously the resort to asylums, viz. the different opinions prevailing among medical men and others in different localities touching what constitutes lunacy, and what may be the character of provision best suited to this or that form of mental disorder.

From each of the reports before us the conclusion becomes obvious that the proportion of pauper lunatics to paupers augments

steadily. Thus, in England in 1859 it stood at 3·66 per cent., whereas in 1874 it reached 6·57 per cent. "In all Scotland it is in the ratio of 8657 to 100,000," that is, over eight and a half per cent. It differs materially in different counties, as already intimated in the extracts from the Scotch report above given. Is not, it may be asked, one chief cause of this relative increase of pauper lunacy to paupers to be found in the growing readiness of people to cast the burden of their insane relatives upon the public rates, and so quit themselves of their cost and trouble?

Such a proceeding implies the addition of a large number of individuals to the roll of insane paupers who, apart from the incidence of their lunacy, would otherwise not appear on it. The lunacy itself is the pauperising agency, either perforce of the circumstances of life, or otherwise from the will of friends, in order to save their pockets, where means of maintenance in private asylums are not wanting, or, in some instances, to secure the patient, as they assume, better accommodation and treatment.

Another outcome of these reports is, that the increase of pauper lunacy is more pronounced in manufacturing counties and in urban populations. This is sufficiently apparent from Table XI (p. 18) of the English report; and it is likewise intimated in the Scottish one, as where (p. 6) the increase of the insane is attributed as "most probably due to the influence of an increasing population, chiefly urban."

The circumstance, moreover, that the character of a population—whether urban or rural, manufacturing or agricultural—largely modifies both the form of insanity and the movements of an asylum population, must always be borne in mind when county asylums are compared together, whether as regards admissions, cures, or deaths. We have already in part illustrated the truth of this fact in the remarks on the relative statistics of Perthshire and Renfrewshire.

"In contrasting the lunacy [observe the Scotch Commissioners] of an active, busy, and increasing population with that of a community less busy, standing still, or, perhaps, even decreasing, two different things are brought into comparison. There can be little doubt that active and acquired insanity is more prevalent among the former, and idiocy and imbecility among the latter; and while acquired insanity may be curable, idiocy once established remains for evermore." (p. xi.)

Further on, they remark:

"That the insanity of a large town represents a very different state of matters from the insanity of a rural district. The former comprises a large proportion of those fatal forms which are the result of the manifold influences comprehended under the general term of fast living, and of which general paralysis is the type; and

the latter a large proportion of those fatal forms which depend on imperfect development and imperfect training, but which are not incompatible with long life. Of these, idiocy is the type." (p. xxii.)

The inferences deducible from these premisses are, that the asylums of agricultural counties should present a larger proportion of imbecile and incurable inmates, and, consequent thereupon, fewer movements of their population and a more rapid rate of accumulation, than do asylums of counties pre-eminently engaged in manufacture and distinguished by large towns.

The proof of these inferences could, if necessary, be illustrated; nevertheless, the results exhibited are more or less affected by various circumstances and conditions. We have already noted the influence of asylum proximity and of the geographical features of a district. Other modifying conditions may be referred to. For instance, immigration into towns withdraws the more enterprising and stronger minds from the country, thereby elevating the ratio of imbeciles in the rural community left behind; at the same time this disturbing influence on the comparative ratio of the rural and urban population is more or less counterbalanced by the less favorable hygienic conditions of town life, and the temptations to dissipation incident thereto, considered as agencies productive of mental disorder. Again, not only do the more acute and fatal forms of insanity prevail among dwellers in cities and demand treatment in asylums, where they consequently magnify both the ratio of admissions and of deaths, but the circumstances themselves of residence in towns compel the removal of lunatics from their houses. On the contrary, among rural populations, owing to the less acute and the incurable forms of insanity prevailing in a higher ratio than in towns, the demand for the asylum treatment is less imperative, whilst, at the same time, the circumstances of their dwellings, their habits and modes of life, make home detention more feasible and practicable.

The mode of distribution of the insane in the two countries may be seen from the following figures. In England on the 1st of January, 1874, there were of the grand total 62,027 of registered lunatics, 30,956 pauper and 415 private in county and borough asylums; 2433 private and 339 pauper in registered hospitals; 3319 private and 1394 pauper in licensed houses; in military hospitals, 358; in workhouses, 15,018; residing with relatives or others, 6839 pauper and 436 private; and in Broadmoor 331 private and 189 pauper criminal inmates. These figures compared with those of the year preceding indicate an increase of just upon a thousand patients in county and borough

asylums, of whom 862 are paupers; an increase in registered hospitals, of licensed houses, and in military hospitals, more especially of private cases; and an increase of 675 in workhouses. As a small set-off against these items of increase, there is a decrease of 218 in the number of lunatics residing with relatives and others. Probably the most important comment to be made on the statistics quoted, and in those at large given in Table I (p. 6), is the seemingly irrepressible growth in the number of workhouse lunatics, notwithstanding the vast and costly additions to asylum accommodation which fails to keep pace with it (see Table XV). The fact is a painful one to reflect upon, as it intimates a progressive multiplication of imbeciles, and incurables, and, *pro tanto*, a progressive degenerescence of the population.

Turning to the Scotch reports we find that, on the 1st of January, 1873 (unfortunately for the purposes of comparison a year behind the date used in the English report), there were 4665 detained in royal district asylums, 342 in private asylums, 1226 in parochial asylums and lunatic wards of poorhouses, 54 in criminal asylums, and 1564 in private dwellings. The pauper lunatics provided for in asylums and poorhouses numbered 4984, and in private dwellings 1488; whilst the private patients found in asylums amounted to 1249, and those placed singly to 76.

The general review of these Scotch statistics is somewhat less unsatisfactory than that of the English. The rate of total increase is slower and the proportion found in poorhouses and parochial asylums is smaller than in England, being as one sixth to one fourth. "Since the 1st of January, 1858, the number of lunatics officially known has increased from 5794 to 7851. In the manner of distribution the following changes have taken place," viz. an increase of 2285 in royal and district asylums, of 387 in parochial asylums and poorhouse wards, and of 28 in the lunatic department of central prison; and, *per contra*, a decrease of 403 in private asylums and 240 in private dwellings. Hence, "these figures show that of the increase of 2672 which has taken place in the number of patients in public and parochial asylums and lunatic wards of poorhouses, 403 arise from the decrease in private asylums, and 240 from the decrease in private dwellings; leaving an increase of 2029 ascribable to the growth of lunacy, or at any rate to the increased number of lunatics in asylums." (p. iii.)

Most important questions hinge on the returns of the admissions, recoveries, and deaths in asylums. The consideration of

these matters furnishes data for arriving at the rate of the increase and of the accumulation of insanity in the country, but it must be conducted with great caution, and a multitude of modifying circumstances be allowed for. An examination of the crude figures alone will lead to various false conclusions.

We have already alluded to many conditions modifying the number of admissions into asylums. But as the number of admissions must constitute the basis for calculating the ratio of recoveries and of deaths, great consideration need be given to the circumstances of admission, to the condition of those admitted, and to the provisions for treatment on and after admission, before we can arrive at results of any real value for the purposes of comparison.

The English Commissioners have constructed several elaborate tables exhibiting the averages of admissions, recoveries, deaths, &c., from which as crude numerical facts we learn that the admissions into county and borough asylums amounted to 9426; that the proportion of readmissions to admissions was 13·13 per cent.; that the recoveries were, as compared with the total admissions, in the proportion of 33·95 per cent.; and "the deaths upon the average daily number resident in the ratio of 10·70 per cent., and, calculated on the total number under treatment, the proportion would be 8·31 per cent. The percentage of recoveries is 2 per cent. below the average of the last fifteen years. The rate of mortality, though higher than in 1872, is somewhat lower than the average of the last fifteen years." (p. 25.)

As concurrent facts, we also learn that there is a further accumulation of chronic and incurable cases, and that, whereas the proportion of patients deemed curable stood on the 1st of January, 1873, at 8·12 per cent. of the numbers resident, it had fallen on the first day of the subsequent year 1874 to 7·39 per cent.

On turning to the statistical history of registered hospitals, we find that the rate of recoveries, as compared with admissions, including transfers, was 40 per cent., and if transfers be excluded 45 per cent.; and that the mortality on the number resident throughout the year was at the rate of 8·96, and on the total numbers under treatment 6·21 per cent., or, excluding transfers, 6·43 per cent.

The painstaking Table XIII and the tables of Appendix (B) represent these ratios for each county and borough asylum and for each registered hospital. And, at the first blush, it would seem an easy and straightforward task to compare one asylum with another, or one hospital with another; or again, asylums with hospitals, and to elicit the truth regarding their

general movements, their success in treatment, and the mortality present in their populations. But, as above intimated, no reliable comparative conclusions could be thus drawn.

In the present and likewise in the foregoing Scottish report (the 15th) great pains are bestowed to show the circumstances to be taken into account in order rightly to read and compare the statistics of recoveries and of mortality in different asylums. Much was said on this head in the article on "Lunacy in England and Scotland" contained in the January (1874) number (pp. 88, 89), which, therefore, need not be restated in the present review. To quote some pertinent general remarks on this matter from the last Scottish report—

"The results afforded by all asylums vary greatly, not only according to the manifold influences which affect the condition of the patients before admission, and which act on their bodily and mental health after admission, but also according to the proportion which the admissions bear to the number resident. A community which receives a large accession of unhealthy members must, it is evident, suffer from a higher mortality than a community of similar size which admits a smaller proportion of unhealthy members; and unless, accordingly, in comparing the results of different asylums, allowance be made for the varying relation which the members admitted bear to the members resident, very erroneous conclusions will be arrived at. The differences which may be found to exist after this allowance has been made may very properly be sought for in the better or worse physical condition of the patients on admission, in the varying nature of their mental affections, or in the more or less appropriate manner in which their wants are afterwards supplied and their treatment is conducted." (p. xxvii.)

The statistics of the parochial asylums of Scotland furnish an instance in which such special features must be allowed for before contrasting them with those of other institutions. Thus, as a bold statistical fact it comes out that both the ratio of recoveries and the ratio of deaths are higher in those asylums than in all other places where the insane are received. That the recoveries are proportionately greater is principally attributable to the larger proportion of the ephemeral forms of insanity they receive; that the mortality is higher is due chiefly to the higher proportion of admissions into them, in comparison with the numbers resident, than into kindred institutions.

To sum up in as few words as possible, the particulars to be borne in mind in deducing comparative statistics are—whatever conditions affect facility of recourse to asylum treatment; whether the asylum be especially rural or especially urban; the climatic and hygienic conditions (for *e.g.* phthisis, as a concomitant malady,

may be fostered by these conditions); the character, habits, food and employments of the people of the district; the relative character of the asylum in respect to its features as a "boarding-house" or a hospital establishment, in other words, the relative length of residence of the inmates; the rules of admission (if excluding certain classes of the insane, *e. g.* idiots, epileptics, paralytics); the ratio of admissions to numbers resident; the average age of those resident; the relative number of curable and incurable cases admitted; the prevailing forms of insanity; the physical condition of those admitted; the relative proportion of the two sexes; the relative number of transfers and of removals of unrecovered patients; and last, not least, the intrinsic conditions of asylums considered as places of cure (to be found in relative size, in extent and efficiency of medical attendance and nursing, and in the many other circumstances affecting health and longevity).

We have chosen to single out the several conditions mentioned as necessarily affecting statistical inferences (and might, indeed, increase the number by more minute analysis of those of a general character), so as to at once catch the eye of the reader, although a majority of them are concurrent and mutually dependent and could on reflection be readily seized upon. Waning space suggests the necessity of bringing our remarks on these interesting lunacy reports to a conclusion. But before doing so we particularly desire to call attention to the thoughtful observations of the Scottish Commissioners on the prevalence of phthisis among the insane, made especially in reference to the high ratio of the malady in the Inverness Asylum.

A general comparative review of the mortality among the insane in England and in Scotland shows that it is in a lower ratio in the latter country. But in Scotland phthisis is a more frequent cause of death, reaching 17·45 per cent. compared with 11·6 per cent. in England. This wide disparity the Commissioners at one place attribute to "differences in domestic hygiene" (p. 268), but in other portions of their report display what we must regard a wiser appreciation of facts, and speak of hygienic conditions as one of several agencies that may be concerned in developing phthisis (as at page 211). The general question of the prevalence of phthisis in asylums is very thoroughly examined by them (pp. lxix—lxxii). We cannot follow in detail the course of their arguments, but their general outline amounts to this—that the average ages of the populations in asylums exceeds that of the general community, so that there is a smaller proportion of young lives; and that, although phthisis is particularly a malady incident to youth and early manhood, it prevails in a much higher proportion among the inmates of

asylums than among the population at large. To get at the relative proportion of phthisis as a cause of death, they point out that it is not enough to calculate the percentage of deaths from that malady to deaths from all causes in the two communities, for this would exhibit no more than the relation which the mortality from consumption bears to the total mortality in the two cases, leaving out of view the amount of the total mortality by which the relation must be influenced.

"The death-rate from phthisis, for instance, may remain stationary, while its relation to the death-rate from all causes may vary considerably." . . . .

"Now, it happens [they proceed to say] that the death-rate in asylums from all causes is no less than four to five times higher than that of the general community, at nearly all ages; and it follows from this that no exact conclusions can be drawn as to the comparative fatality of phthisis in asylums and in the general community from the proportions which the mortality from phthisis bears to the whole community. The mere fact, however, that in spite of the higher asylum death-rate the percentage of death from phthisis to deaths from all causes for the ages between 20 and 35 is as great in asylums as out of them, constitutes a conclusive proof that tubercular diseases are more frequent in them than in the general community." (p. lxxi.)

Accepting the proposition that phthisis prevails more among the insane than the sane, the next point to determine is the comparative frequency of the malady in the two. This the writers of the report proceed to do by calculations—conveniently represented in the form of a table, of the mortality from all causes and from phthisis severally, first for the general population, and then for the population in asylums at the several decennial periods of age between 20 and 90. By way of example of the results obtained, we quote the following paragraph, viz. that "while 1000 people in the general community between the ages of 20 and 30 furnish 4·23 deaths annually from consumption, 1000 asylum patients of the same age furnish 25·15, or six times as many. During the next decennial period phthisis is again nearly six times as fatal to the inmates of asylums as to the general population; during the next, four times; during the next, three times, and so on, being more fatal at all ages except between 80 and 90." (p. lxxii.)

At the Inverness District Asylum there is an alarming mortality from phthisis. "Half of the mortality is due to this malady, and it is noteworthy that it has been much more prevalent among the males than the females" (p. 211). Nevertheless, there is no evidence of numerous admissions of actually consumptive patients to help to explain the fact, for the deaths

are remarked among those who have resided several years in the institution. With a laudable desire to unravel the mystery, the Commissioners pass under review whatever is peculiar about the asylum, its structure and surroundings, its dietary, its ventilation and warming; and pose as a collateral question deserving solution, whether consumption is more prevalent in this than in other districts of Scotland, rightly considering that an answer to this question will enable them to appreciate how far simple removal to the asylum has operated in inducing the malady. In the matter of the intrinsic conditions of the establishment itself, no fault is pointed out in the dietary, in the comfort, cleanliness, and cheerfulness of the institution, or in any details of management; but in the arrangements and apparatus for warming and ventilating the building the Commissioners conclude that "there can be no doubt that therein lies a source of ill-health capable of explaining the great predominance of consumption." As experiments are being made to render those hygienic requirements satisfactory, time will show how far the explanation advanced is to be held sufficient.

It is true this explanation is supplemented by the recognition of minor defects, such as a too frequent scrubbing of the floors and consequent dampness; too enclosed and encumbered airing-courts, involving a deficient access of air and light and interference with proper surface drainage; yet, after assigning every conceivable influence to all the insanitary circumstances noted, we are of opinion that the Commissioners have not reached the bottom of the matter. There are other factors concerned in the evolution of phthisis—circumstances of soil, of exposure, of prevailing winds, of over-crowding, of grouping consumptive patients together, as in the same dormitory, &c., all which will repay investigation, and this not only to the managers and inmates of the Inverness Asylum, but also to those of all kindred institutions.

Other topics of importance are presented in the pages of the two reports before us which, from want of space, cannot be entered upon, but a few memoranda of some official comments and recommendations may be given. The English Board reports well of the general state and management of the county and borough asylums and of the registered hospitals. The licensed houses likewise get a small meed of praise, in that "the general standard at which they are now kept is much higher than it was in the early days of this Commission." (p. 51.)

We are pleased to find they give encouragement to the practical study of the morbid anatomy of insanity, to which we must greatly look for light to dissipate the mystery which

hangs about the malady considered as a bodily disorder. We congratulate the Board on their enlightenment, which by the way contrasts strongly with the condition of another important public body—the Local Government Board, which has just recently pronounced against the practice of post-mortem examinations, and thereby abetted ignorance and prejudice.

The Scottish Commissioners are happily in accord with their English colleagues in this matter, and have the privilege, not only of noting the ardour of some superintendents in pathological investigations, but also of pointing to the institution of a "laboratory of research," as a valuable adjunct to an asylum.

The percentage of deaths in English county and borough asylums exceeds that in hospitals and in private asylums, whereas, in Scotland, the mortality of the establishments last named is somewhat above that of the public asylums.

In the English report we notice, among other recommendations, the proposal to entirely isolate epileptics from the other patients in asylums, and to construct special wards having elaborate arrangements and appliances for observation and watchfulness. We cannot now attempt the discussion of this scheme, but we look upon it to represent an exaggerated want. There are epileptics requiring special isolation, and there are other such unfortunates who do not, and in whose case it would be a hardship, to use the mildest word, to be grouped with the former, and a detriment to their well-being, both mental and physical. Again, the construction of such specially and elaborately contrived wards as they describe and illustrate approvingly after the plan of Mr. C. H. Howell is, to our humble apprehension, at least of doubtful utility, and involves the mistaken policy of attempting to replace intelligent watchfulness and the rightful responsibility of responsible agents by mechanical ingenuity.

The recommendation, again, to erect in asylums a separate mortuary for each sex represents, to our conception, what we may call hyperæsthetic refinement; whilst with regard to the general hypothesis advanced, that unoccupied insane paupers, leading a vegetative existence, require a better diet than able-bodied paupers engaged in active work, we must take exception and hold to be not proven.

Appendix (F), setting forth the boroughs of the kingdom, in order to exhibit the provision they have severally made for their insane poor, or their neglect of such provision, is singularly incomplete in its enumeration—an incompleteness which, without official explanation, is inexplicable.

Dismissing the English report, we will in a few words sum up

all that remains to us that we can here notice in the valuable report of the Scottish Board. They pronounce in favour of small asylums; they object to bringing large numbers of refractory patients together in the same ward, and they are very decided in the opinion "that if our large asylums were so arranged that each ward should become, as it were, a small independent establishment, admitting its patients in rotation as they presented themselves, a very great increase of tranquillity would be likely to follow" (p. li). They perceive no evidence of any change in the forms of lunacy admitted nowadays into asylums, and attribute what is apparent as a milder type to the more enlightened and humane treatment of patients after admission.

The position of single patients and the mode of dealing with them as pursued in Scotland are matters of much interest in connection with the great question of the future provision for the insane. As a class, single patients are rather on the decrease in Scotland as well as in England; but the official reports of their condition made by the Commissioners and their assistants, in the former division of the kingdom, are eminently satisfactory.

The singular pains taken by these public officials in selecting cases for relegation to private dwellings, and the arrangements made by them for the regular supervision of such cases when so disposed of, are circumstances well exhibited in the report before us, and well worthy of attentive consideration, and, in our opinion, also of imitation. The general result arrived at is, that this class of patients is in an improving condition.

X.—Clinical Medicine.[1]

Dr. Foster's 'Clinical Medicine' is a work which, as a whole, cannot but be received by the profession with unqualified approbation. Throughout every page there is expressed in characters so plain that he who runs may read the earnest honest desire of the author to make the most of his opportunities for the benefit and instruction of his professional brethren, as well as his strenuous endeavour to employ to the best advantage his knowledge and abilities for the relief of the suffering which comes under his care. And we beg that Dr. Foster himself, and any others who may read our further remarks upon his work, will accept them in the same spirit—will consider that we are not carping and quibbling at Dr. Foster for not being per-

[1] *Clinical Medicine: Lectures and Essays.* By Balthazar Foster, M.D., F.R.C.P., &c. London, 1874, pp. 364.

fect, but that we, having the same desire as Dr. Foster to do good in our generation, simply view some of his facts from a slightly different stand-point, and therefore think his remarks and conclusions may occasionally be modified with advantage, and with due regard to the same object at which he himself aspires.

In his first lecture Dr. Foster endeavours most urgently to impress upon his readers the importance of rest in the treatment of ulcer of the stomach, and in this he is certainly not more pressing than the exigencies of the condition require. It is reasonable to suppose that any outward movement in any part where an ulcer is healing will interrupt and retard that process; all our surgical experience, acquired from the observations of the healing of ulcers on the external parts of the body, agrees with this. It amounts to a veritable axiom. The only question for us to consider is, whether the method propounded by him is that necessary or best suited for all cases. Dr. Foster's plan for such cases is to give perfect rest to the stomach for one week by feeding the patient during that time by nutrient enemata alone, thereafter completing the cure by a restricted but gradually improved diet by the mouth, and appropriate tonics. The whole lecture is extremely interesting and instructive. We entirely agree with the author that where life is at stake neither physician nor patient ought to permit merely conventional feelings as to decency to interfere with appropriate treatment, but we also hold that these feelings on the part of our patients ought only to be contravened for the most urgent reasons. Decency can hardly be said to be outraged by feeding our patients by the bowel instead of by the mouth, yet it is a proceeding at which many not unnaturally rebel, and to which even the rectum occasionally objects. The experience of Casper has taught us that it would probably be most hazardous to submit a patient already weakened by long-continued dyspepsia, and possibly by hæmorrhage, to a week's complete abstinence; we must therefore nourish our patients somehow and in some measure. We concede that, and we also concede the principle of rest to the stomach, but we deny that it is therefore in all cases necessary to have recourse to nutrient enemata. The recorded experience of Dr. William Hunter proves that the human frame may not only subsist—do its life work, but even recuperate—upon an almost infinitesimal amount of nourishment. It is altogether a question of nursing; with intelligent acquiescence on the part of the patient, and with the aid of a firm and intelligent nurse, it is quite possible to support a patient for a week upon equal quantities of milk and lime water given in half-ounce portions every hour. Such quantities trickle through the stomach into the duodenum, there to be absorbed, without

giving rise either to pain, vomiting, or any uneasiness; and, in our somewhat large experience, we have never had a case in which death occurred after treatment was commenced, nor have we ever had recourse to nutrient enemata for simple ulcer of the stomach. Only rarely have we seen vomiting recur, and then it was clearly traceable to infringement of the regulations. While agreeing, therefore, in the main with all Dr. Foster's remarks on the treatment of simple gastric ulcer—though we observe by the way that he makes no mention of the subcutaneous injection of ergotin as an admirable method of checking hæmorrhage—we yet demur to his dogmatism in the selection of one special means of carrying out his principles. Throughout the whole work there is indeed a strong tendency to dogmatism, which is all very well when confined to the enunciation of principles, but which become narrow-minded and sectarian when extended to methods of treatment and to pathological views. Dr. Foster's excuse is to be found in the fact that most of his essays are clinical lectures reported as actually delivered to students, while all seem to be based upon inquiries made for a similar purpose. But dogmatic teaching, though it impresses for the time, is not favorable to ultimate reputation; even students accustomed to the reiteration of one-sided views, either as to pathology or treatment, though at first they are forced to believe in them by the mere energy with which they are enunciated, yet by and by relegate the teacher of such dogmatism to the class of mile-stones or old fogies, and when such a one appeals to the professional public his ultimate position is still more rapidly reached.

The next essay, on *cyanosis* from a patent foramen ovale, is extremely interesting and well argued, and proves, we think, with perfect conclusiveness that a systolic murmur of a particular character may be due to this cause. Yet even in this essay the same dogmatic character of teaching mars the beauty of the narrative. Thus, no special notice is taken of the many instances in which a patent foramen ovale exists without murmur or cyanosis, nor of those in which even a part or the whole of the septum ventriculorum has been found wanting without pre-existing symptoms; nor, again, is any reference made to the readiness with which a systolic murmur may be produced in children simply by the pressure of the stethoscope, yet all these points ought to be duly considered in any essay on the subject in question, for they have a most important bearing on the signs and symptoms observed.

Furthermore, the tendency to generalise from too limited a series of observations and to criticise others from his own unstable point of view is shown at p. 57, where he expresses

his opinion in relation to a case of cyanosis, accompanied by perfect clearness of intellect, related by Dr. Richardson, remarking that this seemed to have been due to absence of interference with the functions of the lungs, and that the influence of cyanosis on the intellectual powers is always in strict proportion to the quantity of oxidized blood which reaches the brain. But to this there are marked exceptions; in one remarkable case, in which the patient ultimately succumbed to apnœa after having been purple for years from interference with respiration, and with entire absence of cardiac disease except hypertrophy of the right side, there was in our own cognizance perfect intelligence, clear and undisturbed up to the moment of death.

The next paper, on the use of ether in the treatment of phthisis, is a purely practical one, and seems a valuable contribution to therapeutics.

In the following paper, on the use of digitalis in heart disease, Dr. Foster has given a most admirable summary of the action of digitalis in cardiac complaints, and the indications for its use, marred, unfortunately, by what we cannot help, regarding as two most important blunders—he objects to the use of digitalis in aortic insufficiency with deficient compensation, and he praises its usefulness in similar cases with over-compensation. In this view of the action of digitalis we know that Dr. Foster errs with many good men, but we are astonished to find this error homologated by a man of so much intelligence and so large an amount of practical experience. He appeals, it is true, to the sphygmograph, but there is no sphygmograph yet invented of any value in relation to practical therapeutics; it counts the pulse, perhaps, more accurately than the finger, it graphically exhibits irregularities in the duration of each pulsation which might otherwise escape notice, but it can do no more, and perhaps the best proof of this is to be found in the fact that, while Foster finds arterial tension increased by digitalis when it produces diuresis, Lauder Brunton appeals also to the sphygmograph in proof that diuresis produced by digitalis is accompanied by a fall in the arterial tension. In short, the sphygmograph makes pretty pictures, which when they agree with our theories we accept as important, but when they disagree we reject as erroneous, but we cannot rely upon it to teach us anything of value. On the other hand, we know from experiment that digitalis kills with the heart in systole, while the danger in uncompensated aortic insufficiency is death from asystole; does common sense not teach us that a drug with such an action must be invaluable in combating this proclivity, and that any tendency to over-dilatation of the

ventricles is likely to be more than counteracted by the semi-contracted condition in which the walls of the ventricle is permanently kept, a condition which is proved to exist by the increased firmness and force of the heart's impulse, and which occasionally can be measured by the alteration in the position of the apex beat. But to produce these good effects digitalis must be given with no niggard hand; there is no disease of the heart in which digitalis requires to be more freely given, and there is none in which better results are obtained than in inefficiently compensated aortic insufficiency. On the other hand, digitalis is not called for when the lesion is over-compensated; as a rule, it does harm in such cases by increasing the force of the systole, already excessive; when it appears to be beneficial it can be so only by producing so great a persistent contraction of the over-hypertrophied ventricle as to threaten death in systole from tonic spasm of the heart. Foster himself has very accurately pointed out at p. 99 that the state of matters thus produced is most dangerous, and that the drug has very speedily to be withdrawn in such cases. A longer experience and a more enlightened consideration of the mode in which digitalis acts will by and by enable Dr. Foster to correct this error which is, indeed, a grave one. Following this is an admirable lecture on rupture of the aortic valves, and the only objection to any part of this is to the statement at p. 139, that when sufficiently compensated aortic insufficiency has existed for years without inconvenience the original regurgitation must have been slight. This is unquestionably a mistake, as many such cases have been observed in which there was from the first very considerable regurgitation.

The article on the synthesis of acute rheumatism is interesting, but there is a hitch in the reasoning, as the same patient in whom lactic acid produced rheumatism at first was afterwards able to take even larger doses without any untoward result. There is no doubt that in exceptional cases the use of lactic acid is followed by rheumatic affections; in some of these cases the rheumatism is recovered from without pretermitting the use of the acid, and it still remains to be proved that in any of these the lactic acid *qua* lactic acid—and not simply as an accidentally introduced disturbing element, which might have been anything else—was really the active morbific agent.

The paper on Duchenne's paralysis (pseudo-hypertrophic paralysis) contains a very instructive clinical history of three cases of this extraordinary disease, with remarks and a post-script on pathology, referring its origin to lesion of the spinal cord. The next paper is an excellent one on that *bête noir* of the medical profession, the treatment of diabetes mellitus,

which is replete with information and well worthy of careful consideration. In it Foster apparently proves that, in some cases at least, the injection of large quantities of cold water has an appreciable influence in lowering the temperature of diabetics. Following this is an essay illustrating the use of the sphygmograph and cardiograph in the study of diseases of the heart and great vessels. We have already said that no faith is to be put in the sphygmograph, and we fear the cardiograph is not much more trustworthy as a means of diagnosis, and it certainly never gives us any information adequate to repay the trouble and time its application involves. Such instruments can never form part of the armamentarium of an ordinary practitioner; their only profitable use would be to enable us to correct and confirm the meaning of phenomena otherwise ascertained. We fear that the use of such instruments is still wanting in precision enough to enable us even to do this usefully, and is not comparable to the results obtained by long-continued observation followed by a dissection. Given a tracing of a certain form, we can interpet it readily enough by an appeal to a pattern by Marey or some other; but if we procure a tracing of a different pattern, has any of us sufficient confidence in the instrument to base a diagnosis upon it alone? And if not, why waste our time poring over pictures which are fully more likely in the hands of average men to give us wrong ideas as right ones? Let us rather wait till physiologists shall place in our hands instruments which can be used with more precision, and, while so waiting, perfect our own means of diagnosis from palpation, percussion, and auscultation. There is not a single case related by Foster in the diagnosis of which the cardiograph was either required or in which it gave important assistance. The last lecture is a very interesting one on pulmonary embolism following thoracentesis by aspiration, which calls for no remark, as we all well know how often this fatal occurrence follows the most trifling operations.

We conclude as we commenced, with the expression of our qualified approbation of Dr. Foster's work; we have differed from him on certain points of practice and of teaching, but we suppose that even if the angel Gabriel were to publish a work a reviewer would find something to find fault with, and, besides, Dr. Foster is a man of so great ability that it is a pleasure to point out to him where we think he has erred, because we feel sure he will take it in good part, regarding us as merely co-workers with him for the good of mankind, a good to be attained, not so much by the perfecting of ourselves, as by the careful tuition of those who are to come after us, and in this respect there is no higher or more responsible position than that of a teacher of clinical medicine.

Bibliographical Record.

Clowes's Practical Chemistry.[1]—This work is marked out by certain meritorious characteristics from the crowd of recent chemical handbooks. It does not, indeed, contain much new matter, nor are the special features to which the author refers as distinguishing it really novel, but, after all, it derives great value from the intimate practical acquaintance with actual analysis which every page illustrates. If the list of errata had been less formidable, and if the literary style of the work had been more refined, our commendation would have been stronger.

This "Practical Chemistry" is made up of a judicious selection from the text-books of Fresenius, Rose, Valentin, and other writers on the subject, the analytical tables following those of Valentin very closely. Still, the author's experience in teaching has given a special impress and character to his description of experiments and processes.

Mr. Clowes divides his book into seven parts or sections, the first three of which are devoted to the simplest cases of chemical manipulation, such as the preparation of gases, the fitting of apparatus, and the use of the blowpipe. He has certainly succeeded in explaining, with the aid of illustrations, the construction and use of the commoner forms and arrangements of apparatus in so clear a manner that the dullest of students cannot fail to understand his descriptions. The fourth and two succeeding sections discuss the reactions and separations of metals or bases and of acids. One of these sections, the fifth, has been rather spun out, for it simply treats of the analysis of salts containing one metal and one acid, and yet the greater portion of its contents is recapitulated in Section VI. The *tables of differences* given at the end of each of the five metallic groups will prove very serviceable to students, showing, as they do at a glance, the reactions of the metals with the various reagents employed in each group, as well as the behaviour during the Preliminary Examination. These tables ought to be very useful to pupils whose memories are not retentive of chemical facts, to pupils

[1] *An Elementary Treatise on Practical Chemistry.* By F. Clowes. London, 1874. Pp. xviii & 327.

in fact, who belong to the not uncommon *doliocephalic* race—the "sieve-heads" (Latin, *dolium*, a sieve).

The seventh and last section of the work before us contains a list of apparatus and reagents required for the course of analysis; it also gives a series of substances suitable for analysis. Much care has been taken in the preparation of this part of Mr. Clowes's book, and it will be found to afford the means of saving time and trouble to those who have the charge of the working arrangements of a laboratory.

Although it is difficult to give much space to theoretical explanations in a practical treatise, some readers of Mr. Clowes's book may object to his somewhat abrupt introduction of chemical formulæ on page 61, without any preliminary explanation of symbols, equations, and the language of chemical change. But those who raise this objection to books on practical chemistry should remember that the authors of such of these works as are good for anything beyond cramming do not assume that they are to be used alone, but with the aid of lectures and of the intelligent study of chemical theory.

Flint's Practice of Medicine.[1]—This excellent treatise on medicine has acquired for itself in the United States a reputation similar to that enjoyed in England by the admirable lectures on physic by Sir T. Watson. It may not possess the same charm of style, but it has like solidity, the fruit of long and patient observation, and presents kindred moderation and eclecticism.

We have referred to many of the most important chapters and find that the revision spoken of in the preface is a genuine one, and that the author has very fairly brought up his matter to the level of the knowledge of the present day.

Dr. Flint is no believer in the doctrine of change of type in diseases, and has steadfastly upheld the value of bloodletting in some acute inflammations, as, for example, in pleuritis and pneumonia, or, as he prefers to call it, "pneumonitis." Looking to his instructions for treatment, we find a close agreement with those of the best teachers of the British school of medicine, and few illustrations of special American therapeutics.

The work has this great recommendation, that it is in one volume, and therefore will not be so terrifying to the student as the bulky volumes several of our English text-books of medicine have developed into. Moreover, although the pages are closely set, the typography is very good and clear.

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[1] *A Treatise on the Principles and Practice of Medicine, designed for the Use of Practitioners and Students of Medicine.* By Austin Flint, M.D., &c. Fourth edition, carefully revised. 1873. Philadelphia and London.

Roberts's Handbook of Medicine.[1]—In the presence of so many admirable works on medicine, its theory and practice, that have in recent years issued from the press of this country, including works of home-growth, such as Sir T. Watson's, Dr. Aitken's, and Dr. Tanner's, and treatises of foreign production that have commended themselves by their intrinsic merits for translation, such as Trousseau's and Niemeyer's, it would seem a bold and ambitious venture on the part of a young physician to bring out yet another book on that subject, and something by way of justification or apology for the course taken might fairly be demanded, for "of making many books there is no end."

However, the author makes no serious attempt to explain the production of his work, excepting so far as stating that it is addressed especially to students preparing for their examinations, and that he has desired to condense the information needed for this end into a book of moderate dimensions. Its speciality is therefore its being a student's handbook of medicine in one volume, and as such it can be confidently recommended. It is essentially a compilation, and in the matter brought together very completely reflects the present state of medical science, particularly in the departments of symptomatology and pathology. Treatment is dealt with in that general way which suffices the student at a theoretical examination, but which will be found very inadequate to the emergencies of practice.

Dr. Roberts's experience as a clinical teacher has suggested to him two "innovations on the method usually adopted in manuals of medicine," to which he particularly calls attention as special features of his treatise. "First, before describing the individual diseases of the several organs or systems, an outline has been given of the clinical phenomena which indicate a morbid condition of each, and of any modes of 'physical examination' employed in their investigation, while the principal symptoms are considered in detail...Secondly, an endeavour has been made to generalise the remarks on diagnosis, prognosis, and treatment, so far as was practicable."

These innovations will be, we believe, highly appreciated by students, particularly in carrying on clinical work. The information presented is concisely and clearly given, and the subdivision of subject well conceived.

We have to confess to delay in noticing this volume, a delay caused by press of subjects and of books calling for review, but albeit entailing the advantage of affording more opportunity to make ourselves acquainted with the character and merits of the treatise, an opportunity we have used, and which enables us to

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[1] *A Handbook of the Theory and Practice of Medicine.* By F. T. ROBERTS, M.D., &c. London, 1873. Pp. 1043.

speak in terms of praise of its value to students in acquiring the theory of medicine in a manner and to an extent calculated to satisfy the most rigid examiners.

Shapter on Heart and Lung Disease.[1]—With the exception of a short chapter of fifteen pages, the whole of this work is devoted to what is evidently supposed to be a tolerably exhaustive review of the symptoms and signs of cardiac disease, with a few hints as to treatment. Dr. Shapter is an excellent and most estimable physician of nearly fifty years' standing, one, therefore, who was educated long before anything like precision was attained in the estimation of cardiac phenomena, whether healthy or diseased; it is, consequently, a bold step in him to come forward, not with a mere record of experience, which certain parts of this work show him to be admirably qualified to give, but with what, under the modest title of "Notes and Observations," is actually a statement of what he conceives to be the present stand-point of cardiac physiology and diagnosis, and it is from this point of view that, in the interests of medical science, this work must be judged. Dr. Shapter is one who has evidently had a wide field of experience, and who possesses a self-reliant, independent mind; unfortunately, mere amount of experience is no efficient guarantee of accuracy, and mere independence of thought, unless tempered by accuracy of judgment, is more apt to lead astray than to conduce to material progress.

It is now certain that by a careful determination of the rhythm of a murmur—that is, the relation it bears to the several acts which constitute a cardiac pulsation—and by also ascertaining that part of the cardiac area at which it is heard loudest, we can infer with absolute certainty at which of the four cardiac openings a murmur originates, and whether it is a murmur of obstruction or regurgitation.

Dr. Shapter will, probably, be surprised to learn that, though he has devoted a whole chapter to a consideration of the errors in the rhythmical action of the heart, he has very vague ideas himself as to the succession of those acts the continued repetition of which constitutes what we call the rhythmic action of the heart. But how else can we account for his speaking as he does at p. 24 of a systolic murmur which may be so greatly protracted as to interfere with the audibility of the second sound, or, as at p. 48, of an aortic regurgitant murmur which "may so mask and overpower a constrictive (and therefore systolic) aortic murmur as to render it not always easy to decide on the presence of the latter." Systole and diastole, however, are successive and not simultaneous acts of a cardiac pulsation, and a murmur or sound occurring during the one cannot be

[1] *Notes and Observations on Diseases of the Heart and of the Lungs in connexion therewith.* By THOMAS SHAPTER, M.D. London, 1874. Pp. 237.

masked or obscured by a murmur or sound occurring during the other. Very many other examples might be quoted of our author's vague ideas as to cardiac rhythm; for instance, the way in which he speaks at pp. 45, 183, &c., of diastolic and presystolic or auriculo-systolic as signifying the same thing, when we all know they do not, and that there may be an auriculo-diastolic murmur as well as an auriculo-systolic, and that while the latter, from its character and rhythm, is readily enough distinguished from an aortic diastolic as well as—contrary to what our author supposes, vide p. 184 &c. —from a mitral systolic murmur, the former (the auriculo-diastolic) is mainly to be distinguished by its position of maximum intensity, a means of recognising the place of origin of murmurs to which Dr. Shapter has not once referred. Contrary to the experience of all authors who have written on the subject, and are, without exception, agreed upon the fact, that a presystolic murmur is invariably associated with disease of the mitral valves, Dr. Shapter says (p. 189), that whatever the suspicions the existence of this murmur may originate, "it does not absolutely imply vital unsoundness." He has, probably, been led to make this statement by the indefinite results which have followed his own attempts at the diagnosis of a presystolic murmur, but he is as unquestionably wrong in this as in another statement he has made at p. 186, that a mitral murmur is only due to statical causes, "morbid conditions of the blood not appearing adequate to produce it," because the condition of the blood alone is a very frequent cause of mitral murmur even in rheumatism, and still more in erysipelas, chlorosis, anæmia, &c., while a very large number, if not all, of those cases to which Dr. Shapter has referred as instances of an anæmic murmur audible in the pulmonary artery are actually mitral murmurs propagated into the auricle, as Naunyn has pointed out, and having their position of maximum intensity, not in the pulmonary area, but an inch and a half to the left of it, over the appendix of the left auricle.

Dr. Shapter is independent enough to accept as correct the dogma that the diastole of the heart is active, and consequently that muscle elongates as well as contracts; in this he errs, if even it be with many good men both of past and present times; we doubt, however, if there be many either in the past or the present who agree with him in basing the circulation of the blood entirely on ventricular action, to the ignoring of the contractile force of the auricle as well as of those other forces which are in themselves sufficient to produce this end. This, however, is a mere error in physiology, of little importance either in the diagnosis or in the treatment of cardiac disease. But when he argues, as at p. 177, that hypertrophy is not "the statical or mechanical result of valvular disease, but that it has an independent, though probably coexisting, vital origin," he opposes himself to facts with which every observer of any experience

is well acquainted, facts which form the basis of all our treatment of cardiac disease, and upon which much of our diagnosis is founded, because the consecutive changes in the chambers of the heart resulting from valvular lesions are more permanent and therefore more important than the murmurs to which these lesions give rise, which, as Dr. Shapter well knows, frequently vary and occasionally disappear.

Almost every page of our copy of Dr. Shapter's work is covered with marks of exclamation and interrogation, showing how frequently we have felt compelled to differ from him. We regret to have so often to dissent from the views of a man so estimable and of such large experience as Dr. Shapter, but in the interests of truth and of medical science, which are one, it is impossible to do otherwise. The diagnosis of cardiac disease is not based upon mere opinions or on probabilities; it is founded upon immutable and easily learned physical and physiological laws which leave no room for doubt. There is no disease of any organ in the diagnosis of which certainty is more attainable than in valvular cardiac disease; there is none, therefore, in which it is of more importance for us to set our face against those puerile plausibilities which passed current in the early days of our science, and to inculcate the necessity of every would-be teacher, basing his teachings, not on those subjective ideas which he terms his experience and believes he has gathered from nature, but upon objective phenomena, which are capable of proof and which every one can verify for himself.

Brown's Manual of Botany.[1]—The author of this 'Manual of Botany,' who bears the highly honoured name of the most distinguished and philosophic botanist England has produced, has given to students a work which may compare favorably with any of the like kind that have issued from the press. He has evidently sought out from almost every available source, and has brought together within this volume, every opinion and fact deserving attention or admitted within the domain of positive knowledge, and has largely illustrated them by excellent woodcuts.

And although, as he rightly admits, such a treatise must of necessity partake very largely of the character of a compilation, yet Mr. Brown is enabled by his long and varied acquaintance with botany, gained as well by home study as by explorations in many distant parts of the earth's surface in the capacity of botanist attached to expeditions, to critically weigh the statements and opinions of others, to confirm or reject, and also to add.

We have consequently much satisfaction in recommending this manual to botanical students, and, likewise also, in view of the

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[1] *A Manual of Botany, Anatomical and Physiological, for the use of Students.* By Robert Brown, M.A., &c. Edinburgh and London, 1874. Pp. 614.

botanical knowledge at present required of them, to medical students. We however, sympathise with these last in the necessity they are submitted to of "getting up" even so excellent a manual as the one before us for the purpose of obtaining a licence to practice physic and surgery. It is, indeed, a captivating and, except for its terminology, withal an easy science for the learner. Yet, considering the extent of the medical student's curriculum and the exactness and width of knowledge now required of him in every subject, botany should, in our opinion, be only slightly insisted upon either as a matter of study or of examination. Its bearings on practical medicine are few, and vastly few are those who acquire such a knowledge of the science as to turn it to practical account. With the great majority the learning of botany is a "cram," and the amount crammed is beyond utilisation. If the knowledge of the subject is to be insisted on for medical men, that knowledge should be chiefly acquired previously to the course of medical study, for to interpose it in that course and demand the serious study of such a manual as Mr. Brown's, which, by the way, is not so formidable a "manual" as some we are acquainted with, is to intrude upon time and energies that should be occupied in studies more distinctly necessary and professional. These remarks are merely incidental to the subject-matter of the treatise under notice, but are not out of place in a medical periodical.

Nursing the Sick.[1]—People may be said to have first been aroused to the importance of nursing by the appearance of Miss Nightingale's most able little treatise on the subject, which happily may be appealed to still as an authority on almost every detail. Several medical men have since written on nursing the sick, but their productions have not distinctly advanced the knowledge of the matter, being too brief and superficial, and consequently ephemeral in existence. Dr. Munro has now come forward as an instructor in "the science and art of nursing the sick," and has produced a treatise of the sufficient length of 331 pages. The title of his book elevates nursing to the grade of a science; but this position might be readily contested, and most people would be contented with regarding nursing as simply an art.

As a matter of course various smaller details in the management of the sick not alluded to in Miss Nightingale's volume were at the disposal of Dr. Munro for observation, and are not neglected by him, but he sadly wants the incisive and aphoristic style of the lady writer, and, unfortunately, writes in an involved, diffuse, and often ungrammatical manner. Likewise exception might be taken here and there to some of his statements—for instance, to his remarks

[1] *The Science and Art of Nursing the Sick.* By ÆNEAS MUNRO, M.D. Glasgow, 1873.

on the uselessness of packing and on the use of cold baths; whilst those on the diet in child-bed are rather calculated to confuse a learner, who, if taught in the text the advantages of so-called antiphlogistic regimen, has to correct the lesson by the foot-note on the advantages of better living.

However, the chief blemish of the book is its style of composition, for a review of its matter satisfies us that the treatise is replete with useful instruction and suggestions, and that it may be recommended to the happily increasing class of persons interested and concerned in nursing the sick. If a second edition be called for, the whole volume need be carefully revised, and to no inconsiderable degree pruned.

Manuals of Surgical Anatomy.[1]—The almost simultaneous appearance of these two volumes implies the recognition of a want on the part of students for the particular information they profess to give—a want dictated by the increased stringency of examinations, and particularly of those on practical surgery. In fact, until their publication such a want manifestly existed, but now, we are happy to say, it is satisfactorily met. With either of the two volumes the student may rest content, persuaded that he is in the hands of a competent teacher.

The translation of Roser's treatise had slight priority in date of appearance. It is likewise a fuller work, enriched with more details, both in anatomy and surgery; indeed, it may be considered by some unnecessarily profuse in such details, with many of which the student should be familiarised by preparatory training in anatomy and surgery before taking up a course of practical surgery. The fact is, however, that no definite line can be drawn limiting the subject of surgical anatomy, for it is one prone to extension on either hand, in the domain of anatomy or of surgery; whilst, again, the anatomist or the surgeon, in writing on his particular science, may readily trespass on the province of surgical anatomy. Mr. Galton, in his preface, insists on the difference in kind between a manual of surgical anatomy and one of operative surgery on the dead body. He remarks that many look upon the two things "almost as convertible terms, thus failing to recognise the fact that a work such as the latter, all-important though it be, deals only with the carpentry, so to speak, of surgical science, while the former is concerned with the salient points to which the surgeon must ever have regard, as well in the living as in the dead body, if he would frame a correct diagnosis of such and such disease or injury—a preliminary

[1] 1. *The Student's Guide to Surgical Anatomy, intended as an Introduction to Operative Surgery.* By Edward Bellamy, F.R.C.S. London, 1873.
2. *Manual of Surgical Anatomy.* By Prof. W. Roser. Translated from the fourth German edition by John C. Galton, M.A. London, 1873.

which, it seems almost superfluous to state, may in many cases render needless the practice of precepts to the consideration of which a work on purely operative surgery is limited."

We can seize on the meaning of this extract, but we cannot say that it lies on the surface or is clearly conveyed, and we are led by noticing this matter to further observe that the great defect of Mr. Galton's translation is its want of clearness in diction and its abounding Germanisms. By his engagement in the Hessian service as a staff-surgeon during the Franco-Prussian war Mr. Galton would, from the literary composition of this treatise, appear to have lost his English and to have acquired the German idiom. Prof. Roser, moreover, has, as a matter of course, employed both anatomical and surgical terms current in his own country, many of which are peculiar to it. In numerous instances his translator has interpreted these in footnotes, but others have escaped his attention, being doubtless familiar to him as an Anglo-German, and encumber the pages even where they do not embarrass the meaning. It would be in this place to no useful purpose to quote examples; such things mar the literary worth of the treatise, but, fortunately for the student, the value of its matter remains untainted, and may be confidently relied upon, a circumstance amply vouched for by its having passed through four editions in Germany.

Mr. Bellamy's 'Student's Guide' is of rather less extent, but conveys information in a much more readable and comprehensible manner, and quite as fully as the requirements of the student need.

Both works have numerous illustrations, which will materially aid both the reader and the practical worker in surgical anatomy.

Original Communications.

I.—On Granular Disease of the Conjunctiva and Contagious Ophthalmia. By Edward Nettleship, F.R.C.S., Surgeon to the South London Ophthalmic Hospital. Late Clinical Assistant and Curator of the Museum at Moorfields Ophthalmic Hospital.

(*Continued from* vol. liv, p. 460.)

The present distribution of Granular Ophthalmia in London and some other parts of England.

My attention was first closely directed to this disease as a very prevalent one among the children in some of our poor-law schools when I was appointed to take charge of the ophthalmic cases from the Anerley school. Since that time I have taken such opportunities as have occurred of inspecting the eyelids of children in these and in other schools, and at the South London Ophthalmic Hospital. These observations have disclosed some facts which, although familiar to the medical officers of some of the poor-law schools and the army, are I believe not so generally known in this country as is desirable.

Before mentioning any details a few words are necessary as to the standard of health to be adopted, or the line within which variations in the appearance of the conjunctiva are to be looked upon as physiological. There appears to be considerable doubt about this among those who have had the best opportunities of judging. For example, such observers as Thiry and Müller would certainly pass all cases of simple vesicular granulations, even if they had advanced to congestion and some implication of the papillæ, as either normal variations due to differences of original structure or as transient changes caused by slight irritations. Others, *e. g.* Wecker, while taking a similar view on theoretical grounds and refusing to allow that the enlarged normal follicles are the starting-point of "true granulations," do for *practical* purposes join those who assert that enlarged follicles are identical with vesicular granulations. Thus he admits that the former "singularly predispose" to the development of

acute granulations, and, together with all the best observers, advises a strict watch to be kept on this condition of hypertrophy.

On *à priori* grounds we cannot doubt that the conjunctiva, like every other structure, must show, in different individuals and at different times in the same person, some degree of variation in each and all of its normal constituents. Since both lymph-follicles and lymphatics, and tubular mucous glands form parts of the healthy conjunctiva, we may be certain that they will each present such differences. The difficulty is simply the practical one of drawing the line between physiological excess and incipient disease. The word "disease" itself may perhaps in this case have to be used in a relative sense, for it is possible that a variation of the lymph-follicles, and perhaps other parts of the conjunctiva, which might fairly be considered healthy under some circumstances would have to be looked upon as morbid under other conditions, as *e.g.* when large numbers are living together. It will probably be allowed that asymmetry in the degree of an observed variation is strongly in favour of its being morbid. It is well known that, although follicular granulations are almost always symmetrical, they are not by any means always equal in degree on the two sides. This is a great help when it occurs, but when the symmetry is perfect I confess I can see no *clinical* ground for calling a conjunctiva healthy in which any such granulations are distinctly visible. It is impossible clinically, so far as I know, to say whether such granulations are enlarged mucous glands or enlarged lymph-follicles. Nor should I be inclined to take the transient character, or at least changeableness in size sometimes shown by early follicular granulations as a safe guide to their being only physiological variations; because it is most likely that if, in a given person, the follicles were subject to frequent and extreme physiological changes they would be unusually amenable to the action of any cause of disease. It would be a great help towards fixing a normal standard if the opportunity could be had of inspecting the eyelids of children of the upper classes, *e.g.* at some of the higher class boys' and girls' schools; indeed, until this can be done, and done by some one who has similar opportunities among the lower classes, I see no probability of reaching a firm basis. There are moreover some cases which for practical purposes ought to be regarded as having follicular granulations, although these may not be distinctly visible to the naked eye. These are mentioned, generally as cases of "latent granulations," by many authors who have had abundant opportunities of observation (Stromeyer, Bendz, Müller, Hairion). I have seen a few such instances myself and have no doubt that they are tolerably common. In those that have come under my notice there had generally been a rather excessive redness of the whole membrane (but no symptoms or discharge) for a long time before the gra-

nulations appeared, showing probably a high functional activity of every part. The clinical features of such a case are a sudden attack in one or both eyes (generally in one a day or two before the other) of acute conjunctivitis, and the appearance of well-marked and abundant sago-grain granulations in a very short time, perhaps within twenty-four hours. Papillary congestion and a varying amount of muco-purulent discharge will also occur and the case will present no peculiar features in the early stage unless the palpebral conjunctiva be examined. In duration and liability to relapse it will however differ much from a similar conjunctivitis in which there are no granulations. Occasionally, when such an attack occurs in only one eye, the minute vesicular granulations (or lymphatic follicles) may be seen on the conjunctiva of the unaffected eye by the aid of a lens, or may be just visible without its help if looked for with great care. Whatever name is given to such cases, whether they are called "latent granulations," or whether it would not be better to include them under the head of persons whose conjunctival lymph-follicles were in the first instance much developed and became actively diseased on the supervention of a special exciting cause, we must acknowledge their importance and keep a careful watch for their occurrence. In inspecting a large number of poor children a considerable proportion will always be found in whom the conjunctiva of the lower lid is more or less congested and gives the impression of having slight papillary hypertrophy. The membrane may be of a bright tint, but it is oftener of a somewhat dull red or pink hue. In many, perhaps most of them, a few sago-grains can be found on very careful search, but these may be very difficult to find, or if present may be quite hidden. Many of these cases I have been accustomed to pass as healthy although I doubt whether, if judged by the highest attainable standard, they ought to be so regarded.

In the school with which I was connected there were, when I first knew it, about 850 children, of whom about 1 in every 9 or 10 had eyelids which were healthy or nearly so. In from 50 to 60 per cent. of the whole number there were well-marked sago-grains, but without any, or with only a slight, degree of congestion; this number included some in whom the lids were scarred and pale and the disease quite cured. The remainder, about 30 per cent., showed either a more advanced condition of granular disease with or without muco-purulent discharge, or discharge with a slighter form of the granular condition. I do not know to what extent other metropolitan pauper schools resemble the above condition, but as to the general character of the ophthalmic cases under treatment in some of them I know, from personal inspection during the last twelve months, that these were very much like those that I had under my care from Anerley; and in two of them, where I had the opportunity of looking

at some of the children who were not in the infirmary, there were evidently a good many with various degrees of granular ophthalmia, as at Anerley.

Dr. Bridges states that he has "never yet found any pauper school absolutely free" from mixed granular and muco-purulent ophthalmia. It seems highly probable therefore, that an examination of all the children in these schools would show a slight degree of granular disease in a large proportion of the total inmates. As to the children and adolescents of the London poorer classes living at their own homes I have, since my attention has been specially directed to the subject, found abundant sago-grain granulations in a large number of those who have been under my care as hospital out-patients. Many of these are in persons who apply on account of some degree of conjunctivitis, but many are in those who attend for other reasons and some who only come as companions with other patients. A fair proportion of these sago-grain cases are in children who are socially several degrees above the poorest class, their parents being better-class artisans, small trades-people, or clerks; they are children who, as is evident from their dress and general appearance, are pretty comfortably off as regards food, clothing and cleanliness. It is true that so far as I have yet seen, the disease is on the average, less advanced than it was in the majority of the Anerley children. Some of these out-of-door cases, even in well-to-do children who have never been in a workhouse, are however very bad. Dr. Littlejohn, the medical officer of the City of London School, "who has kept careful records of the state of every child upon admission, reports that a very large proportion come into the school with the 'sago-grain' condition well marked, and this whether they have been in the school previously or not" (Bridges).

I have quite lately examined the eyelids of about 350 poor school-children in different parts of a country district in a midland county. The following facts are of interest. Two of these were pauper schools. One of them (which I will call *A*) contained 26 children, of whom 42·3 per cent. had more or less sago-grain granulations. The three children in this school whose lids were most perfectly healthy had been in the workhouse only a few days. The workhouse of which this school forms a part is very healthily situated in an open field, on the top of a hill 800 feet above the sea-level, on a limestone (oolitic) soil. The children were well nourished and on the whole looked healthy. The second school, *B*, was separate from its workhouse. Of its 60 children, 61·6 per cent. had sago-grains on their lower lids. Among the 14 worst of these, 12 had been in the school at least one year, the great majority more than two years. Of the 31 healthy children, 22 had been in the school one year or more. These children are

all natives of a large town on the Thames; the school itself is three miles from the river, about 100 feet above its level, and among the open fields. The children on the whole, looked rather pasty and pale as if they were not enough in the open air, but were well nourished. There have been one or two mild outbreaks of ophthalmia in this school but never any severe or prolonged attacks. The third inspection was at the National School (*a*) of the town to which *A* belonged. In the boys' and girls' school 42·8 per cent. of 70 children showed sago-grain lids more or less marked, while in the infant school (100 children) about 40 were in the same state. In the boys' school one child aged 6, struck me as having exquisitely healthy lids, more so than almost any other. On inquiry I learnt that he was in a better social position than most of the others, was an only child, and was taken special care of. He looked remarkably healthy. Three or four of the infants whose lids were most healthy were also very vigorous looking and were socially above the rest of the children. The town containing these schools has a population of 7000 and is situated nearly as high up as its workhouse (*A*). The inhabitants are a good deal engaged in woollen manufacture and many of the children are more or less employed in helping their parents in the work. The next examination was at the National School of a village of 816 inhabitants (*c*), situated five miles from (*a*), on the same oolitic soil, and about 700 feet above the sea. The parents of these children are all farm labourers. They are now able to live moderately well (wages 14s. a week) except in the matter of milk. This, the clergyman told me, many families never taste, as the milk of only two or three cows in the whole parish is sold by the farmers, the butter-milk being, as a rule, used for the pigs. In the boys' and girls' school 30·4 per cent. of 46 children showed follicular granulations, and the percentage among the 40 children of the infants' school was 32·5. Several of the children were pale, dirty and badly fed, but on the whole they looked more healthy than those at *a*. The only institution of a better class to which I have had access is the London Orphan Asylum at Watford which receives the sons and daughters of persons in the middle and lower middle class between the ages of seven and fifteen. I examined carefully the lids of 100 of these children, some having been many years in the school and a few only about three months. A large proportion (45) of this 100 had well-marked sago-grain granulations on the lower lids and several showed on the upper lids also. The worst cases were in children who had been in the school a long time and had entered it when situated on less healthy premises. The medical officer told me that nothing worse ever occurred than an occasional slight conjunctivitis from cold, &c. It is a very well arranged and well managed institution and the present premises have been built only three

years. The children are well fed and well cared for in all respects. Their dormitory accommodation is exceptionally good, and the only respect in which it struck me that there was room for improvement in the matter of space was in the girls' day-room accommodation. Even this was, however, more spacious than at any pauper school that I have seen. This school would therefore, seem to show that in a well-managed institution, where the children are admitted only at long intervals and where none are ever taken in suffering from contagious ophthalmia, the existence of mild granular ophthalmia is a matter of very little, if any, importance. It is important to note also that in this school, as well as in the pauper and national schools above mentioned, there were no *bad* cases of granular disease. This is in great part attributable to the fact that no outbreaks of severe conjunctivitis had occurred, for no doubt most of the severe cases of granular lids are the result of granular ophthalmia aggravated by repeated attacks of acute conjunctivitis due to contagion or other causes. The well-known fact, however, that pure granular ophthalmia, without any acute attacks, not very unfrequently runs on to bad granular lids, and that such an advanced condition is sometimes absent when the slighter forms are abundant, requires some explanation. In the first place it would seem to show that the disease bears a direct relation in *degree* to the *intensity* with which its cause has acted, no less than to the *duration* of its influence, since it is fair to suppose that the conditions of life are, on the whole, better in the two country schools above described, and certainly in the Orphan Asylum at Watford, than in many of the metropolitan pauper schools. We must, however, remember that the material on which the London schools work is worse as regards general health than that of the other institutions referred to. It is quite certain that, *cæt. par.*, the state of the general health and nutrition exerts a very powerful effect on the progress of the disease in its early stages, and probably in its later stages also.

Prevention of Granular Ophthalmia.—The curative treatment of the disease, being inseparable clinically from that of contagious ophthalmia, does not need separate consideration. For the same reason I shall not, in this part of the subject, make any suggestions for the management of large bodies of children in which granular disease is common, the measures to be adopted having reference largely to the prevention of contagion.

In respect to the prevention of granular ophthalmia in large establishments there is not, perhaps, much that is positively new to be said. In the children themselves the following points require particular attention. The disease not being contagious, so long as it is uncomplicated with muco-purulent discharge, measures taken merely against contagion in institutions where there was no contagious conjunctivitis would be worse than useless, since they would

cause much alarm and expense. Such measures were formerly taken in some countries, especially Belgium, but it is unlikely that they would be recommended in this country.

As to *food*, I see no reason for thinking that its kind or quality has any direct influence in preventing the disease, except in so far as a well-nourished organism is more able to resist the cause of the disease than a badly-nourished one. As to its quantity and quality I believe there is no great reason to fear in our pauper schools. The cooking is not, however, always so good as it might be if a little more care were bestowed. Many are too apt to take for granted that food made of good ingredients must be palatable. I believe that variation of diet, within certain limits, is indirectly of considerable importance to nutrition by increasing the appetite, especially with children the majority of whom never have any holidays outside their school; it is particularly desirable with the very young children. Variation in the cooking is of quite as much importance as change in the articles of food. It is of still more importance that the food when hot should be served really hot, and not lukewarm, especially in the case of boiled meat and puddings, and with the breakfast drink of milk or cocoa. It is also of extreme importance to train children, more particularly infants, against habits of daintiness and wastefulness with their food. Children between three and six years old are very apt to leave a meal (although cut up for them) almost untouched unless they are made or helped to eat it, and many of the elder children are very wasteful unless carefully looked after. The chief deficiency, so far as I have seen, in the *clothing* is the difficulty of washing the boys' cloth clothes. Their waistcoats and jackets get positively filthy from long wear. The trousers, being of corduroy or fustian, can be washed; but the cloth clothes often cannot be properly cleaned without almost coming to pieces, and then they are not worth making up again. There is no doubt whatever that the boys, as a whole, create far more disagreeable smells than the girls, or even the infants. This is, perhaps, because their work and play make them sweat more; but I think it depends also largely on the original offensiveness of cord and fustian (as Carter has pointed out), and on the greater dirtiness of their upper clothes. At Anerley the boys now have summer jackets and trousers of coarse linen which are washed weekly.

As to *ventilation* it is difficult to say anything in connection with these schools which is not a truism, or has not been said often enough already. Since facts, both general and special, point to organic matter impurity of air or moisture of air, or both combined, together with a somewhat high temperature, as causing the disease, I think we must expect to find it more or less prevalent in amount and degree according as the children are allowed to spend a large part of their time under this combination of conditions. In respect to day

arrangements much may be done by introducing out-of-door schooling where as is often the case the schoolroom space is too small; and by allowing more playground space, either in a field or by letting them "fall out" and play about freely when out for walks. The ventilation of dormitories and schoolrooms is a most difficult question when cubic space and means of artificial warmth are both very limited. In the dormitories the necessary evils may be reduced by diminishing, as far as possible, the amount of body clothing in the rooms at night; a strong effort should be made to have this kept in a corridor or separate place at night, where it might be freely ventilated. No chamber utensils at all should be allowed in the rooms, except for children who are known to have dirty habits or to wet their beds. In some schools these children are put together into one dormitory, an excellent plan if only they be allowed at least double the cubic space allotted to others. Young infants also should have more space than elder children because of their dirtier habits, and because they cannot bear so much direct ventilation; artificial warmth is very desirable in their sleeping rooms in cold weather. The greatest amount of direct ventilation that can be borne must be carried out. Practically this can be learned only by experience in each room, and can be maintained only by the constant supervision of some officer who understands both the necessity of fresh air and the danger of draughts. In cold, damp, sunless rooms fires ought to be lighted occasionally, and this is best done on days when they have been scrubbed, so as to dry the floors. It is, I think, a bad thing to allow blinds to be drawn down at night, except in cold weather; in warm weather, when there is not much wind moving, they very much interfere with ventilation. If, in summer, the early light is found to waken the children too early in the morning there can be no other objection to painting some of the window panes than that it makes the room look very ugly.

Contagious Ophthalmia.

It may be taken as certain, without any reasonable doubt, that, whatever the previous state of the conjunctiva, every inflammation of this membrane accompanied by muco-purulent or purulent discharge is contagious.

With respect to purulent ophthalmia, Stellwag (following Piringer) says that the intensity of the contagious power of the discharge varies directly with its degree of purulence and its freshness, and that it is in practice much diminished by free dilution with water, though this does not destroy the power of the individual particles of discharge. Its power is also much lessened by drying. In speaking of the discharge from granular conjunctivitis he says that when watery or merely consisting of turbid mucus it is scarcely

contagious. In the milder forms of conjunctivitis we are bound on *à priori* grounds to suppose that, so far as the conditions are the same, identical statements will be found true; and this assumption is apparently borne out in many cases by clinical evidence of contagion.

There is much difference of opinion as to infection by means of particles suspended in the air. Most of those who have had large experience of severe purulent ophthalmia believe in this mode of communication, especially where many cases of the disease are crowded into the same room with healthy persons. It is far best to assume in practice that this is the case. Some have supposed that the discharge becoming dried and pulverised floats as dust in the air. Others assert that, finding its way down the lachrymal passages, it is blown out "as a spray of contagious particles" (Carter) with the expired breath (Graefe, according to Wells).

There are also differences of opinion as to the effects of transplantation on the intensity of the reproduced disease. Some authors assert that discharge from a mild case of conjunctivitis generally gives rise to a mild form of disease, and in particular that the discharge from catarrhal ophthalmia almost always reproduces the same disease and not a severer form. Others believe that the malady is liable to become more intense, or to change in character, as it spreads from one to another, so that the discharge from catarrhal or from purulent ophthalmia may produce a severer form, and even cause granular lids; and that on the other hand, the discharge from a case of granular lids may reproduce a similar condition or either catarrhal, purulent, or diphtheritic ophthalmia. All agree that discharge from a severe form reproduces a severe form, but some say that in this instance the reproduced disease will tend to be milder than the case from which it was taken.

The uncertainty as to this relation between the producing and reproduced disease is owing in great measure to the fact that too little importance is generally given to the effect of follicular granulations in various stages on the intensity and duration of acute conjunctivitis. One result of this is found in the still current assertion (in the last edition of Wells's 'Treatise,' p. 58), that the discharge from catarrhal ophthalmia may reproduce granular lids, and that the latter may also by contagion again produce granular lids. These assertions, if true at all, are so in a very limited sense; they include perhaps cases of sago-grain granulations originating in the so-called "latent granulations," and others in which prolonged conjunctivitis in a case devoid of sago grains or nearly so, produces at length chronic papillary granulations. In view of the great prevalence of mild degrees of granular ophthalmia, nothing short of an accurate knowledge that the conjunctiva was healthy until the acute conjunctivitis set in will warrant the assertion that granular lids are pro-

duced by the discharge from a case of conjunctivitis. There are many careful observations on record showing that the discharge from conjunctivitis complicated with granulations, although reproducing acute conjunctivitis, is not followed by granular lids unless sago grains existed beforehand. The vagueness with which the title "catarrhal ophthalmia" is used by different authors is another source of uncertainty, for it is probable that if all the cases in which this diagnosis was made by different observers were collected together they would be found to include several well-marked clinical divisions, owning perhaps, very different causes. I shall for this reason avoid the use of the word as much as possible.[1]

Several other circumstances are more or less recognised as having an influence on the severity of the reproduced disease. The patient's age is one of these; *cæt. par.*, the reproduced conjunctivitis will be severer in young than in older children, while it is said by McGregor to be worse in adults than in any children. The state of health also acts powerfully in some cases, though this is a subject while is surrounded with fallacies. Several severe epidemics of the disease in Ireland have occurred during periods of great distress or partial famine. This was the case with the epidemics of 1701, 1772, and of 1848-50. Wilde mentions "debilitated starved female children" as the worst subjects. McGregor says that two thirds of the children under his care in whom the disease caused loss of one or both eyes, had enlarged cervical glands or some other "marks of scrofula," or red hair. There can be no doubt that defective nutrition of the body increases the risks to the cornea, which are always serious enough in bad purulent ophthalmia. Sex appears to bear some relation to the severity of acute conjunctivitis; Wilde found the worst cases in the Irish workhouses among girls, and it has been observed in the London poor-law schools that the girls furnish the largest proportion of very severe cases. Race, probably, has some influence on the character of the inflammation at any rate in the case of the diphtheritic form, this being extremely rare in England and common in some parts of Germany. How far the degree and character of pre-

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[1] The following would perhaps be some of the chief groups:

1. Conjunctivitis from exposure to cold wind. Acute, mild, very liable to relapse; not accompanied by conjunctival ecchymoses; not always symmetrical.

2. Conjunctivitis from dust and other irritants or from foreign bodies too small to be identified.

3. Conjunctivitis which sometimes accompanies acute catarrh of the upper air passages, whether this occur alone or as a part of measles or influenza.

4. Acute symmetrical conjunctivitis accompanied by intense congestion and often by conjunctival ecchymoses; getting well spontaneously and often affecting several persons in the same house at nearly the same time. Mr. Hutchinson teaches that this form resembles the specific exanthems, and that it occurs irrespective of age, health, or temperament, and, as a rule, only once in a lifetime.

valent conjunctivitis are subject to the influence of general causes, independent of the source of the contagium and the state of the recipient, it is very difficult to say. Such influences, supposed or real, are spoken of by many authors under the titles of "catarrhal atmospheric constitution," "endemic constitution," "change of type," while others indicate without naming it. While far from denying the possibility of some general cause outside the organism which may stamp the character of the disease in many patients at or about the same time, I think no such hypothesis should be allowed until an effort has been made to avoid certain obvious sources of fallacy. It is easy, for example, to attribute to such a cause phenomena which may be explained by the prevalence of epidemic influenza or measles; by prevalent distress or famine causing a great aggregation of debilitated people; or even by some more local peculiarities, such as temporary overcrowding, the accidental accumulation of many feeble and unhealthy children in a particular school, the introduction of a very severe case from without, recent changes of management, and many other local circumstances.

In the metropolitan poor-law schools it is certain that a large proportion of the cases of muco-purulent conjunctivitis occur in children who were previously the subjects of more or less advanced granular disease. The proportion of the latter is probably smaller, but still considerable, in the children of the London poor living in their own homes; it may be expected to be much less among the poor of healthy country districts, and to be a relatively rare condition among those of the middle and upper classes; but the two last positions require proof.

It may be useful next to give a short account, such as my experience furnishes, of the chief varieties of mixed conjunctivitis and granular ophthalmia, which in the aggregate make up the bulk of what is often erroneously called "school ophthalmia," or "army ophthalmia," or "Egyptian ophthalmia." These terms are apt to mislead, and it is better to discontinue their use.

1. In examining a number of children with different degrees of granular disease some will generally be found in whom there is a very small quantity of opaque yellowish discharge. This is present in most cases either as a string or flake on the inside of the lower lid, generally far back, or else it is rolled up into a little yellow pellet which sticks at the inner canthus. Whether it is in all cases muco-purulent, or whether it may not in some cases be simply "turbid mucus" (Stellwag), without any recent pus-cells, I do not know. Practically this is of no consequence since it is a distinction which can be made only by a careful microscopic examination, and even then there might be some room for doubt; it is best to consider turbid mucus on the conjunctiva as muco-pus. This state of things always coexists with a considerably granular and congested state of

the lids, at least of the lower lids, and these cases have already been mentioned under granular ophthalmia. I have said before that many of them if watched for a long time will be found to remain stationary, or to oscillate slightly, but never become worse as to discharge than is indicated in the above description. I mention them in this place again, however, partly in order to include every case which can possibly be supposed contagious, and partly also to remark that it is impossible to say certainly from a single inspection whether a case in this state will remain quiet or not. I believe, from considerable observation, that the practical contagiousness of such cases *when not progressive* is *nil;* and I think that the observer's action with regard to them should depend greatly upon whether he has or has not opportunities for personally inspecting them at short intervals. If he has such opportunities then these cases need not be separated from children who are healthy, or who have uncomplicated follicular granulations; but if more than a few days intervene between the inspections they should, as a preliminary step, be isolated. Such as never show more discharge may, after several months, be restored to the healthy children. Pathologically it seems right to include such stationary cases as belonging to pure granular ophthalmia with increased mucous or slightly muco-purulent secretion the result of chronic inflammation. With regard to those which now and then get a little worse the same is probably true, if we add the action of some local irritant which has temporarily increased the congestion and secretion; this view is supported by such exacerbations being often confined to one eye. As, however, there is no practical good to be derived from this view, it is safest to treat such variable cases as if they were just within the limits of contagious ophthalmia.

2. Other children will be found in whom there is constantly, or almost constantly, rather more discharge than in those of the first division. Sometimes their lids are glued together in the morning; sometimes the discharge will dry among the lashes into hard, yellow, semi-transparent lumps, or it may remain moist either among the lashes or at the inner canthus. It is often to a large extent mucous, often partly meibomian secretion, and probably its contagious power is small. It accumulates during the night, and often does not collect in the daytime to any perceptible extent, so that if well washed off in the morning the eyes may appear quite healthy to a superficial examination. Most cases of this kind are in young children from 2½ to 7 or 8 years old. The lower lids show either evident sago-grains, or a more or less congested and thickened state by which these may easily be hidden if present. The discharge however, does not bear any fixed relation in different children to the condition of the lids, particularly in very young children; in these the conjunctiva occasionally shows very slight signs of disease, either

follicular or papillary, although a notable quantity of discharge may form during the night. The discharge in this class of cases varies from time to time without any assignable cause, being sometimes all but absent for many days and then reappearing, but never going beyond moderate limits and not being accompanied by any subjective symptoms. Variations of temperature and moisture may have some influence on the discharge. It is a question whether some of these cases do not arise and last a long time without the presence of follicular granulations, and if this is the case it is probable that the disease may, when very chronic, give rise to permanent thickening of all the conjunctival structures and to papillary granulations. Sago-grain granulations may of course be formed under the influence of their own cause while this chronic slight conjunctivitis is in progress, and very probably this coincidence has among other reasons given rise to the theory that they are the result of the inflammation or of the contagion which may have caused the conjunctivitis. While holding firmly that follicular granulations are the result of a cause which does not in the first instance act on the other parts of the conjunctiva, I should think it highly probable that any condition of chronic hyperæmia would aid its action. As a good many of these cases of slight chronic discharging ophthalmia probably go undetected and uninterfered with I think it has still to be determined whether or not they have a share in giving rise to the chronic changes, especially papillary changes, found in elder children. I do not know to what cause many of the cases here indicated are due. When the discharge is accompanied by proportionate development of sago-grains and by considerable papillary hypertrophy, it is easy to account for it by the action of sundry slight local irritants, especially dirt and cold air, added to the naturally high functional activity of the parts in young children. When the discharge is out of proportion to the structural changes it is probable that the disease is comparatively recent, and has not had time to give rise to chronic thickening, but whether in these cases it originated in contagion or not I do not know; from the mildness of the cases I should doubt contagion, and should be inclined to think that the disease is due to cold, damp, irritation by dirt, dust, &c.

3. A tolerably well-marked set of cases will be found in which, if treatment be omitted, a gradual relapse of all the symptoms takes place, until the child is found to have profuse, thick, yellow discharge and more or less congestion of the ocular conjunctiva, with or without some œdema of the lids; sometimes there is photophobia and pain, sometimes these symptoms are absent. The symptoms and course in these cases are tolerably definite and can be predicted with some certainty. There are always well-marked follicular granulations on the lower and often on the upper lids, with great congestion and thickening. By treatment the state of the lids can be

much improved, but to a degree which varies with different cases although nearly constant in the same patient; the discharge can either be cured altogether or reduced to a very small quantity (with occasional exceptions, for here and there it cannot be reduced beyond a moderate amount of chiefly mucous discharge); the congestion of the eyeball can be completely or all but removed. This state of nearly complete cure can be kept up for an indefinite time by treatment. At length the treatment, having been carried on for many weeks after the above state has been reached, is omitted. After an interval, varying from a few days to several weeks, the conjunctiva of the lids, which had been almost or quite as pale as in health and more or less smooth according to circumstances, gradually reddens; the granulations, sago-grain and papillary alike, grow slightly from day to day; the discharge slowly increases; the large remote vessels of the ocular conjunctiva by degrees enlarge more, and this congestion extends day by day to the smaller ones and to those nearer the cornea until the exposed part of the eye has a dull reddish colour when seen at a few feet distance. Sometimes very quickly (in a single day), at others after a few days, all the symptoms increase until the state described at the head of the paragraph is reached. If treatment is now resumed the symptoms quickly disappear and the case is soon as well as before; or if this be adopted so soon as signs of relapse are noticed the later symptoms will not be developed. With cases of this kind the same thing happens repeatedly, so that it is necessary to have them constantly under watch when not under treatment. They can always be kept well by treatment. The duration of the cure appears to vary directly with the total duration of the case, and with the time during which treatment was continued after the cessation of symptoms. After repeated courses of treatment the relapses also tend to become less severe; but this cannot be relied upon. Cases of this kind, if not treated early are very liable to the supervention of severe photophobia and to ulceration of the cornea, especially of its upper part. If ulceration has occurred in a former attack it is very likely to occur again.

There are cases, which must be included in this group, where the course is not quite as above described, the relapse being much more rapid and more severe. In some of these the rapidity of onset and intensity of the symptoms vary in the same case at different relapses without any evident cause.

4. Very many cases of granular ophthalmia, with or without much habitual discharge, can like those of the above class, be kept well or nearly so, by treatment, but are very subject to *slight* attacks or relapses of conjunctivitis, varying in intensity and in character, but never severe. No sharp line can be drawn between some of the worst of these and the less severe cases of the former

group; yet I think on the whole the distinction here made is a useful one, separating cases known to be very liable to severe relapses and serious secondary mischief from those which show a very much smaller liability to these. The attacks here referred to are often sudden, or if gradual are at first so slight as to attract no attention. They are often unilateral throughout and always so at first. The second eye when affected begins to suffer from one to three or four days after the first and is often attacked more mildly than the first.[1] Sometimes they pass off spontaneously, but many get worse and some serious, if left without treatment. They are generally characterised by the appearance one morning of more or less congestion of the ocular conjunctiva, with or without slight increase or renewal of discharge. The congestion is often at first confined to the part of the conjunctiva opposite the palpebral fissure and in this feature the cases differ from those relapses which begin with gradually increasing congestion of the concealed part of the globe; it quickly becomes universal affecting the small as well as the large vessels, and by the latter feature is distinguished from the distension of the straggling larger vessels noticed as forming an early symptom of the more gradual relapses. In most cases the subconjunctival vessels are also involved, and in a few well-marked instances they are congested much more than those of the conjunctiva, so that the eyeball has a pink instead of a brick-red or mixed tint. Sometimes these attacks are complicated with conjunctival phlyctenulæ, the same patient sometimes having phlyctenulæ during one attack or in one eye, and not in the other eye or another attack. Many children have several of these attacks while not on treatment; they occur now and then, but much less often, while local treatment is being pursued. Generally no cause can be ascertained, but sometimes the children mention "cold" or "dust." Scarcely to be separated from these are certain children who are very liable to slight attacks of conjunctivitis from exposure to strong winds, especially, I think, easterly winds. These will be mentioned again further on.

5. Severe first attacks and relapses of inflammation are sometimes set up in granulous patients by foreign bodies on the conjunctiva and blows on or near the eye.

6. The last group calling for separate attention is that of corneal ulcerations. The ulceration being generally due to the granular state of the upper lid usually occurs on the upper part of the cornea, is superficial and accompanied by the formation of blood-vessels. It generally occupies about the upper third, or less, of the cornea, and often sends towards the centre a more or less pointed

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[1] This fact, which is a common one also in the severer forms of disease, is supposed by Stellwag to indicate diminution of intensity by transplantation.

process of ulceration which may cover part of the pupil even though the lateral parts of the ulceration are considerably above the horizontal diameter. Ulcerated corneal phlyctenulæ are also far from uncommon in this disease, and in severity and duration are sometimes quite out of proportion to the state of the lids; when this is the case the corneal disease probably has nothing to do with the existence of granular lids. The same remark probably applies to small, isolated, greyish, nearly central ulcers which sometimes form and are apt to recur. Allusion has before been made to those cases in which the whole cornea becomes facetted and more or less hazy from the effects of repeated superficial ulcers. These are very often associated with severe old ophthalmia tarsi, and abundant, florid, fine papillary granulations, sometimes without any visible sago-grains.[1]

Causes of Contagious Ophthalmia.—Though every form of purulent and muco-purulent ophthalmia is communicable by contagion, there is no form except gonorrhœal ophthalmia and perhaps ophthalmia of newborn infants which may not arise from other causes. It is scarcely needful at the present day to insist on the necessity of making full allowance in practice both for the actual and possible share taken by contagion in causing these diseases, more particularly in large establishments. But it is not enough to recognise only this cause, however much stress we lay upon it. By giving too large a share to contagion as a cause it is possible both to produce some needless alarm and to pass over other influences which account for a good many cases.

There can be no doubt, from clinical evidence, that epidemics of severe purulent forms of ophthalmia, however they originate, spread by contagion in all cases. In the extension of this, the most serious form of epidemic ophthalmia, contagion un-

[1] The following facts as to relapses of all kinds which took place among about 375 children, between May 20 and Dec. 22, 1873, may be of interest.

Total number of Relapses, 215.—Of these, 134 occurred in 53 patients, giving an average of 2·5 relapses to each. The remaining 81 were, for the most part, single attacks.

Relation to Previous Treatment.—Relapses occurring while under strong local treatment 46 (of which number, 9 were due to some special exciting cause which was identified). Of the 32 patients who furnished these 46 attacks, 10 had 14 attacks at other times when not under treatment. Relapses occurring while under mild astringent treatment, 9 (very few of the children were on such treatment). Relapses occurring during omission of all treatment, 160 (total = 215).

Interval between Omission amd Relapse in 160 cases.—Two to seven days, 38; seven to fourteen days, 90; fourteen to twenty-one days, 17; twenty-one to twenty-eight days, 5; more than twenty-eight days, 10 (= 160).

Duration of Relapses.—In those coming on while under strong treatment (average of 24 cases in which it was noted) = 5·62 days. In those coming on while not under treatment (average of 72 in which it was noted = 7·62 days. Many in which the duration was not noted were very short.

Spontaneous Recovery from Relapse.—This occurred in 13 relapses, the average duration in 10 of them (where this point was noted) being 6 days. These were all very mild cases.

doubtedly plays a part which eclipses all other causes. The definite belief in the contagiousness of purulent ophthalmia dates from since the Egyptian campaign, although there seems to have been some popular faith in it before that time; and the merit of first proving it by copious clinical details, in 1802, was claimed by Edmonston, surgeon to the regiment which first brought the disease from Egypt to England. The novelty of the opinion at that time is curiously shown by Edmonston's almost apologetic conclusion: " . . . . yet (he says) the adoption of the opinion, that the Egyptian ophthalmia is infectious, may have its use." As early as 1789 Dr. James Armstrong recorded the details of a violent epidemic on board an English war ship, after the addition to the crew of three sailors suffering from it who were impressed from a slave ship (in which both slaves and crew were affected) in the West Indies. Edmonston quotes this as the earliest circumstantial account of contagion. Power, writing only a year later than Edmonston, and himself to some extent a contagionist, refers to the belief in contagion as existing in England so early as 1790 when "a species of ophthalmia" was prevalent, and says that "a species of it is (1803) frequently prevalent among the Irish peasantry, which is considered by them to be infectious." It is I believe, certain that no suspicion of this cause was entertained when they left Europe by the great majority, if by any of the medical officers, either English or foreign, who served in the Egyptian campaign, nor did this belief exist in Egypt at that time. Many years later (1825) Mr. Abernethy said he had been fully persuaded that purulent ophthalmia was contagious "long before our troops went to Alexandria," and again, "it was always considered so (*i. e.* catching), and people used to talk about catching sore eyes." McGregor in 1804 published some accurate evidence of contagion when the disease occurred among the children of the Royal Military Asylum. This was followed, in 1807, by accounts from Peach and Vetch of the disease and its contagious nature as it occurred from July 1805 to 1806 in the second battalion, 52nd Regiment, at Hythe and other barracks in Kent. Its introduction into this regiment was attributed at the time to the entrance of a recruit or recruits from an Irish militia regiment, which had suffered severely from it in Ireland and was said to have taken it from the 79th Infantry Regiment. The last-named corps brought the disease from Egypt to Ireland, and the militia regiment above mentioned had in Ireland been garrisoned with it. But the doctrine of contagion was far from universally accepted for many years, for as late as 1826 Mr. Lawrence treats it as only probable.

It has already been mentioned that purulent ophthalmia when concentrated probably may be and is communicated through the air; this occurs at distances of perhaps a few feet, as from bed to

bed. This was believed by the earlier contagionists and supported on their part by many most instructive facts; it has been held in turn by most of those who have had to do with epidemics of the severe disease and is still the generally received opinion. It is of course very difficult to prove, but we shall do well to assume its truth when a good many cases of ophthalmia, producing abundant purulent or muco-purulent discharge, are placed in a confined space with healthy persons. I disbelieve strongly, however, that conjunctivitis can be communicated in this way unless the discharge is very abundant, nor is it at all probable that articles of clothing or towels hanging in rooms so occupied would filter out so much of the discharge from the air as to become potent sources of contagion, or even practically contagious in any degree at all. The disease stands, perhaps, midway between those which are very easily communicable through the air, and such a one as porrigo (contagious impetigo), which requires the direct transmission of a particle of discharge by the fingers or towel &c., to a healthy skin. It cannot, in this respect, be fairly compared with ring-worm, where the contagious material is dry and powdery or scurfy.

So far as I have seen, the clinical evidence of contagion as a cause of mild and moderate attacks or relapses of muco-purulent ophthalmia in a carefully conducted school is very small. In the school infirmary which was under my charge, there were many cases of uncomplicated granular ophthalmia in very various stages, a good number of them having but little discharge. There was also a moderate number in whom more or less of granular disease was complicated with considerable discharge. Although, as a rule, the cases with most discharge slept in the same wards, it was impossible for practical reasons to fill any ward with such cases, so that they were always mixed at night with a certain proportion of others in whom there was little or no discharge. In the daytime the cases having most discharge were kept in their wards (not in bed except in rare cases), and were then often mixed with those who were warded for other reasons. This mixture was even greater among the youngest children, whose ward it was necessary to convert into a day nursery where not only some children with discharge, but others with none remained for many hours in bad weather. This somewhat promiscuous arrangement of the children was, as far as possible, neutralised by strict precautions intended to prevent the direct transmission of discharge from one to another. These were chiefly the strict provision of a separate towel and separate basin for each child (the towel being hung at the bed-head), and the plan of having the eyelids of every child thoroughly cleaned with small pieces of wet tow the first thing in the morning, each bit of tow being used only once and then thrown into a special receptacle and afterwards burnt. If acute conjunctivitis with moderate muco-

purulent discharge, were so contagious as is sometimes implied in general statements about ophthalmia in schools, I should probably have had numerous cases of more or less severe disease originating and showing evidence that they were due to contagion, notwithstanding the precautions above described. I should add that the measures (which were as stringent as circumstances allowed) were taken at a time when I fully believed in the doctrines of the extreme contagionists and was under the belief that contagion would be an everyday event unless they were taken, and that it might even occur independently of them. I have very gradually, and almost unwillingly, come to believe that contagion is not a very frequent cause of first attacks or relapses of muco-purulent ophthalmia in institutions under tolerably good management.[1] The hypothesis of contagion is so simple and explains the facts so well, that knowing its possibility we are easily led to assume its actual occurrence in many cases which are due to other causes. I have seen very few cases in which there was any good reason to believe in its occurrence, and I believe that in such cases as I have had under care, and as are common in similar institutions, contagion is not by any means always the cause. Welch holds very similar views with regard to cases of the same or even greater severity. In speaking of conjunctivitis in men with follicular granulations he says that in only three cases, among a large number of which he is writing, was there evidence of positive purulent inoculation, and that in no less than 68 per cent. of the cases there was "no ascribed cause." He is strongly of opinion that contagion has a very small share either in originating or maintaining the comparatively mild ophthalmia which now supplies the bulk of the army cases. Among the principal reasons for this belief he mentions that the disease is not eradicated by measures based chiefly on the hypothesis of contagion, and that on the other hand, it often does not spread from chronic cases to healthy men under conditions favorable to contagion. He thinks that contagion has a large share in spreading it when there is much overcrowding, but that there are many other important causes which will continue to act after the facilities for contagion have been abolished. Dr. Pontus has made a somewhat similar statement, based on the result of his experience in a large military house of detention. He found with surprise, that a considerable number of men with mucous discharge, and a few in whom it was muco-purulent, lived and slept among the healthy men without fresh or

[1] It will be objected that schools under "tolerably good management" do not furnish much ophthalmia. True; contagion doubtless accounts largely for ophthalmia, when abundant; but in a pauper school where follicular granulations are very common, there will probably always be some acute and chronic muco-purulent cases, even when contagion is rendered practically impossible.

acute cases arising, and that this state of things had been going on more or less for sixteen or seventeen years.

My own opinion being based very largely on old cases, and on the evidence afforded by relapses, may perhaps be objected to on the ground that many cases which I attribute to relapse are really due to fresh contagion. It has been already mentioned that under good conditions as to cubic space, cleanliness and precautionary measures many children with uncomplicated though severe granular ophthalmia in various stages may live among a number of others, some of whom have a good deal of discharge, without ever getting an acute attack. I have also stated above that those cases which relapse preserve as a rule, the same characters throughout all their relapses, so that in each patient the character of the relapse can often be predicted; and further, that in many cases the relapse comes on gradually, beginning in a slow increase of the roughness of the lids. It is especially with the worst cases that I have carefully observed this, and it is just these which would most likely be caused by contagion if this were in operation. Again, the worst relapses generally occur in those cases which have, when not under treatment, had the most discharge; it is but seldom that a bad attack occurs in a child who at first had a scanty discharge, except in cases of old scarred and seamed lids where some definite cause of irritation (cold draught from a window open at night, a blow on the eye, or a foreign body) brings on a violent attack with ulceration of cornea. In slighter relapses, when the ocular congestion comes on suddenly, or appears to do so, it is impossible to disprove contagion, except by the negative evidence that it would probably cause much severer attacks than these. Another reason against this cause is to be found in the not infrequent occurrence of relapses in groups which have no reference to the arrangement of the children in their dormitories, or to their special occupations, but which are sometimes related to sudden changes of weather, or to the children having all been out for a walk on a windy day. This experience is borne out by what I have seen at another school (a very well-managed one) where some cases of moderate relapse, or of chronic muco-purulent ophthalmia with granular lids, were allowed to mix with healthy children without any harm.

Not more than three or four cases occurred under my charge in which there seemed any good ground for thinking the attack due to contagion, and in several of these there was great doubt about it; they were all severe. The only instance in which there could be no doubt was that of a newly appointed nurse, who, coming fresh to the work and not having learnt or attended to my directions, herself began to suffer from acute ophthalmia in the right eye within three or four days of her arrival. The disease made rapid progress and was accompanied by severe œdema of the lids and abundant dis-

charge, and the other eye then became affected. It was soon cured and her lids became perfectly healthy in about three weeks. The second eye was not so severely affected as the first, probably because treatment was begun earlier in it. I could not, even in this case, trace any contagion; the woman denied knowledge of it, and the cases under her care were all or nearly all mild as to discharge. Another very suspicious case was that of a young kitten which had slept under the bed-clothes of a girl who had bad granular lids and was subject to occasional attacks of bad conjunctivitis. The kitten after sleeping with her for two or three nights showed symptoms of acute ophthalmia in one eye; it never became very severe, and did not affect the other eye.

The commonest vehicles of contagion are probably fingers, towels, and pocket-handkerchiefs. To these must be added sheets and pillow-cases if more than one child sleeps in each bed. Next to these probably come water and washing utensils. There are well-authenticated cases of gonorrhœal ophthalmia being communicated by the use of a towel soiled by urethral discharge. It is difficult in practice to distinguish the share taken by foul water and basins from that due to towels, and notwithstanding the experiments of Piringer which show that the discharge is rendered much less contagious by free dilution with water, I think there should be no relaxation of vigilance in this particular, especially as it is said that the water acts only by lessening the adhesive power of the discharge and not by destroying its efficacy. I never found any evidence of intentional inoculation, nor could I hear of any cases from the nurses or other officers. It is probably a very rare occurrence, if it is ever done at all.[1] Flies are considered by some authors (McGregor, Anagnostakis, Burton) to be an important vehicle of contagion, especially in hot countries. This can take place only where, in consequence of extremely dirty habits, the discharge is allowed, as in Egypt, to accumulate about the eyelids.

The contagious material need not be derived from the conjunctiva. The best known instances in which it is furnished by other surfaces are of course gonorrhœal ophthalmia and ophthalmia neonatorum. Edmonston has known "a severe ophthalmia to be produced in a child from bringing into contact with the eyes the matter adhering to the dressings of a foul ulcer." Ware relates a case of purulent ophthalmia, followed by slough of cornea, in a

[1] Three children were at various times reported to me for having purposely tried to aggravate their ophthalmia by rubbing their eyes, &c. One was probably untrue, as the patient was a stupid, lethargic boy, who appeared to care very little what was done or said to him. One was an intelligent girl who wished to remain in the ophthalmic school instead of returning to Anerley, and was reported to have rubbed her eye to make it red just before I examined her for the purpose of seeing whether she was fit to return. The third, also a girl, was said to have said that she would do the same, but I never found that she did it.

woman who for several days before the attack began had been frequently syringing her two little girls who were suffering from vaginal discharge. Sperino believes he has proved that severe purulent ophthalmia is sometimes produced in children by the discharge from impetiginous blepharitis and from impetiginous eczema. Experimental inoculations have been made with discharge from purulent ophthalmia in various stages, on the conjunctiva of the same and of different persons, by several surgeons. Mackesy experimented on himself with a negative result (1816). Guillié (1824) proved that the discharge of purulent ophthalmia neonatorum reproduced the same disease in other children of ten to fourteen years old, one of those experimented upon having already lost both eyes by the disease in infancy. Piringer (1841) showed that purulent ophthalmia was communicable from one eye to the other, and from the urethra to the eye. Later, gonorrhœa in both men and women was experimentally produced by inoculation with discharge from ophthalmia neonatorum (Landau and others), and from purulent ophthalmia of adults (Thiry); and each observer saw a second gonorrhœa caused either experimentally or in the usual way by these primary cases. Thiry has seen a mild urethritis follow inoculation with discharge from a mild ("catarrhal") form of ophthalmia.

With respect to the lower animals purulent ophthalmia was caused in dogs and cats by the discharge from the same disease in man, by Vasani in 1816, and I believe by Graefe about the same time. Buzzi, in 1825, obtained similar results with the same discharge in dogs, goats, and sparrows. De Condé (about 1841) observed violent ophthalmia, leading to blindness, in a sergeant's dog which habitually slept in its master's bed, the sergeant himself having lost an eye from the disease and still having much discharge. In another case, mentioned by the same author, a little boy suffered from bad purulent ophthalmia soon after being locked up for punishment in a kennel with a dog which was the subject of "extremely violent" ophthalmia; careful examination revealed no other possible source of contagion. Animals therefore are a possible source of contagion.

Other causes.—If we deny the first place to contagion in causing primary attacks and relapses of moderate muco-purulent ophthalmia, except under conditions of very great overcrowding and very bad management, it is not easy to point to those which generally do give rise to these cases. A large proportion of the relapses are I believe due, as has been already said, simply to the gradual sprouting, without any exciting cause, of the chronically diseased conjunctiva which had been repressed by treatment. In some, on the other hand, the conjunctiva having become under treatment nearly or quite smooth and pale, will remain so for a long time, and if occasionally examined will not show any retrogression towards

disease, when from a slight cause a violent relapse will occur. The same slight causes sometimes bring on first attacks, and many of the less severe relapses can be traced to one of them. Of such slight causes ("slight" because the effects produced are out of all proportion to those which would follow their action on healthy eyelids), those which have come most under my notice are the following:—*Slight blows* in the neighbourhood of the eye or on the eyeball caused violent purulent ophthalmia with great œdema of the lids in three children. All were cases of old granular lids and in two of them the lids were scarred; two of them had previously suffered from bad attacks, and the third probably. The same cause brought on a first attack of tolerably severe and very persistent muco-purulent ophthalmia in a little girl of five who had previously had red lower lids and perhaps "latent granulations;" abundant sago grains rapidly appeared in her lids. *Draughts from over-ventilation at night* caused bad relapses in two or three cases of old granular lid in which there had formerly been corneal ulceration, and several slighter ones were attributed by the patients to this cause. *Strong wind during the daytime,* especially while out for a walk, brought on several bad relapses in old cases and many slighter attacks, some of these being primary. I think a cold dry wind is the most dangerous, but warm, moist, gusty winds are by no means free from risk. I am inclined to think it is chiefly the wind, and only in a less degree the dust raised by it in dry weather, to which these relapses are due.[1]

Foreign bodies and dust.—It is difficult to ascertain how many cases are due to these causes. It is not uncommon in examining a number of children with uncomplicated granulations to find several with an eyelash, or a particle of dirt or of coal, on the lower lid without the child being aware of it. There is often increased redness of the eyelid and a flake of yellow discharge, while these are absent from the opposite eye; but these disappear as soon as the cause is removed and no further trouble ensues. A good many children, on the other hand, who have various degrees of relapse,

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[1] On the cold, dry, windy, and dusty days of March, April, and May, it was a common thing for several relapses to occur in a day, especially after the children had been for a walk. 33 such cases occurred on eighteen days in March, April, and May. But a considerable group also occurred during damp, gusty weather in the early part of December (14 cases in five successive days). During these same days in December 8 other people in the house had various slight ailments supposed to be due to "colds." As to the effect of very damp, foggy, rather cold weather 10 cases occurred on four such days, during two of which 3 other persons had stiffnecks; while in eleven days of warm, clear, balmy (? moist) weather without wind or rain, in April, only 4 cases happened. These facts, so far as they go, bear out the statements made by authors that cold damp air is apt to bring on relapses, and that the same is likely to be the result of strong, gusty winds, whether accompanied by dust or not, though it is probable that dust adds to the bad effect of the wind.

will tell you that the attack began from getting some dust or dirt into the eye. I have no doubt that very often the subjective feeling of grittiness caused by the roughening conjunctiva does duty for a foreign body. In a few instances there seemed satisfactory evidence that a foreign body had begun the attack. In one of these a tolerably severe first attack in one eye was caused by a minute chip of coal; the chip did not injure nor remain on the cornea. This boy had previously had a moderate number of scarcely perceptible follicular granulations with moderate congestion ("latent granulations"); these rapidly enlarged and remained in this state for many weeks, and were still the same when the boy passed from under my care; his other eye was not attacked.[1] It is worth noticing that this boy's sister, who had been under treatment before I was in charge and had follicular granulations when I first saw her, had a violent relapse of muco-purulent ophthalmia in both eyes from no assignable cause several months before her brother came under care; at the time I thought it due to contagion, but had no positive reason to suspect this. She remained subject to relapses, which more than once again threatened to become severe. Both these children were sanguine brunettes with thin skin, and very intelligent. In two or three others relapses were attributed to particles of bath-brick while cleaning knives, and in one to chloride of lime. Soap[2] running into the eyes while washing, or rubbed in by another boy, caused rather bad relapses in three cases. On the whole, fine dust such as is raised by sweeping rooms and passages, or even yards, does not appear to have done much harm; at least I could never distinctly trace relapses to this cause. It is to be noted that all the six cases in which a foreign body (dust or soap) was most probably the cause had a great tendency to relapse, as shown by this having already occurred at least twice before in four of them, and once in one other, while subsequent relapses took place in three or four of them.

There can be no doubt that in very sandy and dusty countries—Malta, Egypt, Algeria, &c.—the dust is a much more important cause of acute conjunctivitis than in this country. The finer prickles of a kind of cactus which float in the air at a certain time of the year are stated to be one cause of ophthalmia in Algeria (Furnari).

Measles.—The experience of thirty cases of measles which occurred in the months of August and September showed that it has, as might be expected, a bad effect on cases of granular ophthalmia. The conjunctivitis which accompanies measles sometimes persists for

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[1] I saw him again several months later and found abundant large sago-grains on *both* lower lids, the advance of the disease in the second eye being most marked but having occurred without any acute attack as in the eye first affected.

[2] Bad soap is mentioned by Dr. Edward Smith as a cause of ophthalmia in schools.

many weeks afterwards with a good deal of discharge and an increased roughness of the lids. Troublesome phlyctenulæ also sometimes come on. In a brother and sister, who before had shown a very moderately though decidedly granular condition with little discharge, the lids became much thicker and redder and the discharge abundant; these symptoms persisted in spite of treatment and of long periods of omission for several months. These two children were alike in features, and both had very thick loose lips and large mouths. According to Kriebel an epidemic of ophthalmia in a school for sons of soldiers at Annaburg was caused, in the autumn of 1825, by the appearance of measles. It is stated that many of the children who were not under treatment for ophthalmia had conjunctival granulations.

Other causes mentioned by authors and of considerable importance are the following. *Rapid lowering of temperature at night.*—Many of the early writers on Egyptian ophthalmia stated that exposure of the head to the air during sleep at night was a very common cause of acute ophthalmia. It is said that the same often occurs in Malta where some persons have an attack every time they sleep in the open air, and that it is a frequent cause of an ophthalmia which becomes epidemic every year at Aleppo. It is repeatedly stated that those escape who take the precaution to cover their heads when sleeping in the open air, or to sleep under cover. It is to be observed that in the countries where this happens there is often heavy dew and a great fall of temperature at night. Massy is strongly of opinion that among the soldiers at Athlone acute attacks or relapses of "severe conjunctivitis" were often caused by mounting guard on a cold, exposed spot at night, directly after coming from a hot, close guard-room. He says it may come on so severely in two hours as to disable a man from continuing on duty. Change from the hot guard-room to cold night air is the only cause of relapses which Welch was able to identify with certainty in his regiment. Cooling of the extremities during rapid fall of temperature at night is a secondary cause in Egypt, according to Anagnostakis.

Damp.—In speaking of moisture of air in relation to granular ophthalmia, it has already been mentioned that one source of fallacy in respect to its share in that disease is the fact that it undoubtedly has an influence in the production of some forms of acute conjunctivitis. While it is probable that moist air at a *high* temperature has much influence on the development of granular ophthalmia, its effect in causing acute conjunctivitis is in all probability, due to a rapid *lowering* of the temperature of the air and of some part of the body. This is commonly stated in the case of hot climates like Egypt and Malta. In a sandy, but damp, foggy part of Demerara, Chisholm tells us that both natives and Europeans were, in 1794, notoriously subject to *ophthalmia membranarum.* Redmond men-

tions a good instance of wet causing cold and thus relapse, in which thirty men with ophthalmia sleeping in a tent became much worse after a single night's violent rain which obliged them to leave their beds and stand in the middle of the tent to avoid getting drenched. Vetch also states that regiments stationed in some very damp parts of Kent suffered more severely from ophthalmia than almost any others in the kingdom. De Condé mentions a severe epidemic in a temporary camp on a low part of the shore near Naples in 1815, where the wind was very damp. In all these cases, whatever effect is attributed to the damp, is probably in reality due to rapid lowering of temperature. A few authors attribute considerable influence to hot moist air; *e.g.* in Egypt during the Nile flood, acute ophthalmia, according to many writers, becomes much more frequent, while other catarrhal complaints do not show any increase (Anagnostakis). Redmond, writing of ophthalmia in a British regiment stationed in Jamaica in 1808, says, "I have observed that the hottest weather, accompanied with a dry atmosphere, had such a powerful effect in diminishing its violence as to reduce our numbers at one time to twelve convalescents; but the rainy season setting in, the admissions became daily increased and more violent." In Malta, Hennen thinks the hot moist sirocco brings on many attacks, and sometimes causes a deterioration of all the ophthalmic patients in a single night when it begins to blow. He gives details, showing the number of acute cases and relapses admitted in two successive weeks, during one of which the dry winds prevailed while the hot, moist sirocco blew every day of the other week; the numbers are eight in the former and twenty in the latter. It may be doubted, however, whether much of the effect is not due even in these cases either directly to cold air or to evaporation from the surface of the body, for in the same countries the nights are often spoken of as remarkably cold. Vetch appears to attribute the bad effect produced on cases of vascular cornea by air saturated with moisture to the prevention of evaporation from the vessels, so that these remain distended; he says that these cases always get worse in such weather and require more care in the application of remedies. This observation may be of value in connection with Müller's statement that the conjunctiva is susceptible of perspiration and inhalation. Careful observation may show that it explains some of the sudden and, at present, quite unaccountable changes which take place, either for better or worse, in the degree of vascular turgescence and accompanying corneal infiltration in some cases of partial pannus.

Overcrowding.—Most authors agree that destructive purulent ophthalmia may arise under conditions of great overcrowding, without the introduction of a severe case from without. The most typical instances are supposed to be those frightful attacks of the disease which used sometimes to spread through the slave ships, and

of which an instance is given by Armstrong as happening about 1789 and two others by Guillié in 1819. Most of the epidemics in which many severe cases occurred, in transport ships, barracks, schools and workhouses, have been coincident with great, sometimes intense, overcrowding. The overcrowding in Cairo is stated by Anagnostakis to be almost incredible (1857). These facts do not show that crowding can produce purulent conjunctivitis any more than the occurrence of smallpox under the same conditions proves its spontaneous origin. For obvious reasons it is impossible in such circumstances to say that there were not at first some cases of old granular lids; and if this be granted we have at once a starting-point for purulent ophthalmia, for every one knows that this condition is very apt to be set up in such patients by various causes when there is no overcrowding in question. Among a large collection of men it would be almost impossible to be sure that some were not suffering from gonorrhœa in some stage, another source of contagion. The known facts in the cases of temporary crowding can therefore easily be accounted for by the increased facilities for contagion which are thus afforded. In permanently overcrowded buildings we must add the probable presence of many persons who being already the subjects of granular ophthalmia in various stages and degrees, are thereby rendered very liable to bad conjunctivitis.

Prevalent mild Ophthalmia.—Many epidemics of severe ophthalmia in regiments, schools, &c., have been traced not to any single imported case of severe disease, but to the existence of a prevalent mild form of conjunctivitis in the establishment or district for a short time before the severe disease began. There are many circumstantial accounts of such occurrences on a large and small scale, and it is probable that this cause has now and always has had a larger share in the production of destructive epidemics both in armies and schools than the importation of severe ophthalmia from Egypt. We are constantly told that in Egypt itself acute conjunctivitis varies much in prevalence at different seasons, and in both prevalence and severity in different years. Such mild prevalent ophthalmiæ are generally called "catarrhal" by authors and the cases are usually described as of short duration, to which are sometimes added more details, such as rapid onset, symmetry, the occurrence of conjunctival ecchymoses, &c. Sometimes they are attributed to cold winds, at others to an "epidemic catarrhal constitution" appearing in different forms in other patients, at others to a prevailing epidemic of influenza. Epidemics of this nature sometimes cease entirely in a few weeks; many instances of this are on record. In other cases they form starting-points for prolonged and severe ophthalmia which sticks to the regiment or school for years. Such a result is to be attributed, 1st, to the previous existence of abundant follicular granulations among the patients, by which each case

is much prolonged and often rendered much more severe; 2ndly, to facilities for contagion; 3rdly, to a prevalent state of low health, which, apart from the presence of granular lids, probably makes the conjunctiva more susceptible to causes of inflammation. It may be remarked here that an examination of the early history of severe ophthalmia in the British army renders it nearly certain that the epidemic influenza of 1803 acting on Irishmen, who (civil and military alike) were predisposed to bad ophthalmia by follicular granulations, had a larger share in the production of the destructive ophthalmia which raged after that date than cases imported from Egypt. Gonorrhœal ophthalmia, no doubt also took a share, but it is impossible to say how much. These causes were much aided by facilities for contagion due to the introduction of the barrack system and to increase in the size of the army.

Treatment.

Before beginning to treat mixed granular and contagious ophthalmia it is very necessary to form an idea as to the effect for which we ought to hope. This is a point on which we are, I think, apt either to be over-sanguine on the one hand, or on the contrary, finding the matter a more troublesome one than was anticipated, to rely too much on the expectant plan.

Conjunctivitis unassociated with follicular granulations, or with the complex granular lid of which these are the earliest stage, is as a rule easily curable and has no tendency to occur again without a renewal of its exciting cause. Conjunctivitis occurring in a person with granular lids is not so easily cured and is very liable to be excited again by extremely slight causes. Further, the acute inflammatory attack often adds much, not only to the temporary activity of the chronic granular disease, but to its permanent development. The wave of acute disease leaves behind it an increased growth of the morbid tissue and a permanently increased functional activity prone to show itself by further growth. The chronic tissue-change constituting the granular disease will cease spontaneously after a time, but this end may be much hastened, the granular condition kept much in check and the accompanying risks much lessened by local treatment. The objects to be gained by local treatment are therefore first, to cure acute conjunctivitis, both for the patient's own sake and to prevent it from spreading by contagion; second, by checking or so to speak starving the granular disease to diminish its prospect of leading to corneal damage while in progress, or of becoming bad enough to cause deep and extensive scarring of the lids, and by keeping the conjunctiva in an anæmic and quiet state to lessen its susceptibility to the various causes of inflammation.

The attacks of acute inflammation speaking in general terms can be cured by astringents. By the same means the chronic tissue-changes of the granular disease can be kept in check and gradually diminished. This constitutes, on the whole, by far the safest plan of treatment; but we may, in addition, set up at tolerably short intervals a certain degree of acute inflammation, as a result of which some of the morbid tissue will generally be absorbed. The merely astringent plan sometimes has no effect on granular lids; while the weak points in the second method are found in its liability to set up too great an inflammation, and thus to increase instead of diminish the disease, and in the impossibility of predicting, from the mere appearance of the conjunctiva, whether this will happen or not. When, as is the case in the treatment of large numbers, a certain amount of routine practice is almost unavoidable, I think it will always be found better to rely on the astringent plan for the bulk of the treatment, reserving the powerful or caustic applications for such cases as require and can receive more separate attention.

In the selection of astringents we may be guided by several considerations. The treatment of acute conjunctivitis may consist either in the use of a weak solution often or a strong application at long intervals. In the severer and invaliding cases it does not much matter, so far as the patient's time and occupation are concerned, which plan we adopt. Probably the best and shortest cures will always on the average be obtained by a combination of the two plans, because the weak solution can be made, when used freely, to serve the double purpose of a cleansing and a therapeutic agent, whilst the use once in twenty-four hours (sometimes twice in very bad cases) of a powerful agent will shorten the attack very much and lessen the risk of corneal damage.

For the treatment of granular lids without acute conjunctivitis, weak astringents are of little or no use; indeed, they sometimes act as irritants and increase the disease. But if experience were conclusive as to their efficiency, it would still be found much better to use a strong remedy seldom than a weak one often. The course of granular ophthalmia is so long that it is of great importance to prevent the treatment from taking up more of the patient's time and attention than absolutely necessary. This is obvious enough in the case of adults, and a little consideration will show that it is no less true for children. It is no uncommon thing for a child with granular lids to have from two or three to a dozen attacks of ophthalmia requiring treatment in three or four years, or a shorter time; these if treated by weak applications often last for many weeks, or even several months, so that such children not only lose a large part of their time idle in the infirmary, but are subject to the disheartening process of having constantly to learn subjects over again in school which they have half learned, perhaps, several times before.

Besides the actual loss of school-time there is, of course, very often a great gain of bad habits, the result simply of idleness. It is, therefore, of great importance that treatment in all cases which admit of it—and they form a very large proportion of the whole—should interfere as little as possible with the ordinary school work. This object can be gained only by reducing the number of applications to a minimum, and making sure that when practised they are efficient.

The next step is the selection of cases which require treatment, and this is not quite so simple a matter as it seems at first sight. There can be no doubt as to cases of acute conjunctivitis. These, however slight, should always be treated or at least sent to the infirmary, because, however slight such a case may be, we cannot always tell by the early symptoms whether it will be severe or not, and whether it will pass off spontaneously or not. Then, all cases should be treated in which there is persistent discharge (unless this be only a very little mucus or watery material), whether the lids are granular or not, and all cases of ophthalmia (sycosis) tarsi. It is in the cases of granular lids without discharge, whether acute attacks have already occurred or not, that it is difficult to decide on the propriety of treatment. This difficulty arises, first, from the want of fixed relations between the subjective and objective symptoms; secondly, from imperfect knowledge as to the final result of natural cure in moderate or even bad granular disease unaccompanied by subjective symptoms; thirdly, by the impossibility of predicting accurately how much and what kind of effect any given treatment will have until it has been tried; and, fourthly, from the similar difficulty of saying, merely from the appearance, what are the risks of relapse or of acute conjunctivitis occurring if the case is left alone.

It may be safely laid down as a rule that treatment should be carefully tried for a long time in all cases where there is intolerance of light or spasm of the lids, and of course where there is active corneal disease. There is some difference of opinion and much difference of practice, however, in the case of severe uncomplicated granular lids without symptoms or discharge, or with, perhaps, a little mucous or watery discharge. I think that all the best experience is in favour of treatment for most of these cases—first, in order to reduce their prospects of acute inflammation being set up by slight causes; secondly, to lessen the probability of corneal damage by the mechanical irritation of the rough lids; thirdly, to diminish the prospect of remote bad effects on the curvature, &c., of the lids. All these objects are attained when by treatment the congestion and roughness of the palpebral conjunctiva can be reduced and its irritability diminished. There is less concurrence of opinion in favour of treating early granular disease without much or any congestion or thickening of conjunctiva, and without symptoms. If the effects of treatment on these cases were certain to be good it would clearly be

right to treat them all, but in the slight and early forms of the disease there is so much probability of provoking by treatment just such an acute conjunctivitis as we are labouring to avoid, that I think the patients will as a rule be more likely to escape future attacks of inflammation and aggravations of the granular state if left alone, *in moderately good hygienic conditions*, than if subjected to local medication. I should apply this rule at least to large collections of such cases; under circumstances where a good deal of attention can be given both by patient and surgeon, more of them may, perhaps, be treated with advantage.

There will be a few exceptions to some of the groups indicated above. There are some cases of granular lids with chronic discharge, chiefly of a watery and mucous character but easily becoming purulent, in which no treatment, either by weak or strong astringents or caustics, in whatever form, either in solution or ointment or solid, does any good, but, on the contrary often does harm. The same now and then occurs, but less often, with cases of granular lids accompanied by persistent photophobia, with or without some discharge. In some of these atropine is a help—in some it is of no use at all. These exceptional cases are all the more puzzling because sometimes it appears that the same treatment which did harm at one time does good some weeks or months later, or *vice versâ*, without any apparent change having taken place in the mean time.

The effects to be expected from local treatment.—One of the most important rules to be remembered here is that (excepting as regards the cure of acute conjunctivitis) there is no uniform state of improvement to which all cases of granular disease of the same severity can be brought by treatment. In this respect every case has its own standard beyond which it cannot be carried, and this can be found with safety and certainty only by experience in each instance. In some patients the congestion and roughness can be entirely removed [1] from the upper lids, and almost from the lower, and this state be maintained indefinitely by treatment. Below this point of excellence are all degrees of variation down to the point at which the most powerful treatment that seems safe has no effect at all beyond causing transient irritation; this complete want of effect seldom if ever occurs except in very old cases. Running parallel with cases that are benefited are a few others in whom the symptoms point to the necessity of treatment but which are positively injured by treatment of any activity; these are not numerous.

Duration of Treatment.—This also can be determined only by experience in each case. It can seldom be measured by anything

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[1] I believe, however, that the normal brightness can never be restored to a conjunctiva which has once been granular, probably because it does not really regain the smoothness of health.

less than months, and many cases require it at intervals for years. It is of much importance not to omit treatment entirely until several weeks (four to eight) after the case has reached the greatest amount of improvement of which it is capable. This should be done gradually, *i. e.* by increasing the intervals between treatment; and after its entire omission the lids should be examined at intervals of about a week, so that any gradual sprouting of the conjunctiva may be detected early. I attribute some relapses in the cases under my care to a partial neglect of this rule, for scarcely any relapses occurred when all the worst cases were under treatment without any experimental omissions.

It does not seem likely that anything will be found equal to nitrate of silver and sulphate of copper as local applications. When properly used they give results which, taking the nature of the disease into consideration, leave little to be desired. For general purposes there is nothing equal to a solution of nitrate of silver of 10 grains to the ounce of water, in which strength it is chiefly or wholly astringent. Ridgway, in 1812, came to the conclusion that this strength was the best, and precisely the same solution was strongly recommended by O'Halloran in 1824, who chose it after a prolonged trial of solutions varying from 1 to 30 grains in the ounce. Welch in 1869 still advised it. It is in common use at Moorfields. This solution applied daily with a moderately large camel's hair brush (the common penny brushes) cuts short most inflammatory attacks with great certainty and very much reduces the roughness and redness of the lids in the chronic granular condition, unless this be of very old standing. In a great many cases nothing else is required. After using it for several weeks on a number of children it will be found however that some of them do not improve either as to discharge or the state of the lids. With these either a solution of 20 grains to the ounce may be used, or the mitigated solid nitrate. If the want of effect has reference chiefly to discharge I prefer the 20-grain solution. When, however, the case is one of old thick granular lids the mitigated caustic often has more and better effect, especially as its action can be regulated with considerable nicety. The solutions of the above strength should not be washed off from the conjunctiva unless in a few very irritable cases, and such will generally not be fit cases for treatment by silver at all. The solid diluted nitrate of silver should at first be washed off after 5—10 seconds with a solution of salt (5 grains to the ounce), or of chloride of zinc (2 grains to the ounce), either with a syringe or a large soft brush; but it will be found after a few weeks that in many cases the application thus made has little effect, and it will be necessary either to wait from 15 to 30 seconds or more after applying the caustic before washing it away, or to omit washing it off altogether. The last procedure is perfectly safe in a great many

cases from the beginning; but as, if used in this way to all the cases, there is risk of producing increased irritation and corneal ulceration in a certain proportion, the more cautious plan safer. The difference between the effect of washing off the caustic and not doing so may often be advantageously studied by using one method to the right eye and the other to the left of the same patient, supposing that the disease was at first symmetrical in degree. The difference in the amount of improvement is sometimes very markedly in favour of not washing off the caustic, while in others (the more irritable ones) these conditions are reversed. The mitigated caustic should at first never be used oftener than three times a week, though some old cases will be found in which its daily application is necessary. The 10-grain solution must be used daily or three times a week, according to the effect it is found to produce. In applying these solutions I think it is much the best to invert both upper and lower lids and allow a large drop to flow over them, accompanying this with one or two gentle movements of the brush so as to distribute it equally; it is of importance to carry the brush high up above the upper edge of the tarsus in the upper lid, or this part, which is generally the seat of considerable structural change, will often escape contact with the solution. The same must be done in most cases with the solid caustic, though in this case more care is requisite in order not to touch the cornea; this accident (which has happened to me two, or perhaps three times) can be avoided partly by keeping the point of the caustic forwards instead of backwards and thus slightly lifting the upper lid away from the globe, and partly by pressing the inverted lower lid backwards and upwards, so as to intervene for a short distance between the globe and the upper lid.

Solutions of more than twenty grains to the ounce are unsafe unless washed off, and when this is done they have no advantages over the solid form, and are indeed, less easy to localise than the latter.[1] As a rule I prefer a solution to the solid caustic for young children, who often struggle a great deal, and for those elder children who are unruly; there is some risk of touching the cornea with the solid pencil if the head be suddenly jerked, and this cannot always be avoided even when the child is held between the knees.

A certain number of patients do not improve under the use of silver in any form, nor even when used at long intervals; a few others are made decidedly worse by it, the congestion being permanently increased, the granulations growing rapidly and bleeding on

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[1] Dr. Hairion, however, advises the use of a *concentrated* solution with a *sable-hair* brush, oil being afterwards applied to the conjunctiva. I do not find any statement of the precise strength of his solution. Sable-hair is stiffer than camel's-hair and thus better adapted for applying a minute quantity of solution to a limited part of the conjunctiva.

the slightest touch.[1] Most of these are cases which derive much benefit from solid diluted sulphate of copper (lapis divinus) applied two or three times a week, or oftener, according to experience. Such as do not improve under either silver or copper will not, so far as I have seen, do so with preparations of either lead or tannin or tannate of lead. I have not, however, made any extensive or very prolonged trial with these substances, with the exception of the solid acetate of lead of which I shall say more below. Sulphate of copper is not, I think, any safer for average cases than the 10-grain solution of silver, while it is on the whole less efficacious and not so easy to apply, especially to young children. There are some cases in which it does decided harm of the same kind as sometimes follows the silver preparations, and my impression is that these occur oftener under the use of copper than that of silver. For these reasons I prefer the solutions of silver as standard applications, reserving the lapis divinus for those who get worse or do not improve under any form of silver. It is a curious fact, which I have several times observed, that children who are made worse by either of these substances are generally benefited in a marked degree by the other. I have been less struck with the necessity of changing the treatment than some other observers, the only decided impression I have on this point being that after no more improvement can be gained by silver, however varied in frequency or degree, the lapis divinus does sometimes carry the case considerably further.

There is no objection to the 10-grain solution of silver being applied by a careful and skilful nurse who has been taught how to use it. This may be quite safely done with a large number of the cases, provided that the medical officer sees them himself at intervals of a few days so as to watch their progress. This is another practical reason in favour of this solution over the lapis divinus, with which in effect it is perhaps most comparable and the use of which could not be left indiscriminately to a nurse.

I have made many careful trials of the solid powdered acetate of lead,[2] both according to the method of Buys, and the modified plan of Gouzeé which consists of applying the salt in a slightly moistened state instead of in dry powder. I applied acetate of lead in more

[1] In respect to the use of silver in the early period of phlyctenular ophthalmia (whether conjunctival or corneal) complicating granular lids I have not been able to come to any fixed rule. There is no doubt whatever that it often cuts short an attack, and it is equally certain that in a good many it increases the irritation. I think that in the same patient it generally has the same effect during different attacks.

[2] It should be mentioned that most of my cases were treated with the ordinary acetate which is acid, not with the neutral acetate as advised by Buys. The only neutral salt I could obtain was supplied under the name of "trisacetate;" it was so sparingly soluble that it would not adhere to the conjunctiva at all, and had no effect on the six cases in which I used it.

than fifty cases, repeating it two or three times in several of these. The general result was that, when used on Buys' plan, the ordinary (acid) acetate (sugar of lead) had in most cases no effect at all on the granulations beyond whitening them for many months. It slightly increased the discharge when this had been present before, while in a few cases it did decided harm by leading after some months to ulceration of the cornea, due, no doubt, to the apices of the granulations being rendered harder and rougher by its presence than they would have been if left alone. When applied on Gouzeé's method I found that it did harm in a much larger proportion of cases, especially by causing ulceration of the conjunctiva followed by the formation of cicatricial bands which often imprisoned small lumps of the salt beneath them; these rough, projecting nodules and scars often after several weeks or months set up or predispose to relapses of conjunctivitis, and in several cases ulceration of the cornea was produced by them. In one child slight organic entropion occurred, which was, however, easily remedied by removing a horizontal fold of skin. I think, therefore, that the use of powdered salts of lead in the treatment of granular lids should be definitely abandoned as having no good effect and often a very bad one.

After the application of preparations of silver and copper cold fomentations should be used if there is much pain; they greatly relieve the present pain and the risk (which is generally greatest, I think, when the pain is most severe) of too great inflammatory action setting in.

General management.—This has reference to the general hygiene of the institution in which the children live, to their health, to the avoidance of the causes both of granular disease and of contagious conjunctivitis, and to the general management of those who require constant or intermittent treatment. It is unnecessary to enter into details on the subjects of health and hygiene, since these ought to be raised to as high a standard as possible for every reason, and not solely for the prevention of ophthalmia.

Of the second class of considerations it is needful to go minutely only into the question of preventing contagion. The avoidance of the other causes, although very important, as I have implied by giving them a prominent position, is both obvious and, at the same time, often more difficult to accomplish than this one. Although I have said that I do not think contagion enters so largely as is generally supposed into the causation of any except purulent ophthalmia, and perhaps some occasionally prevalent forms of "catarrhal" ophthalmia, it is none the less necessary that all possible precautions against contagion should be taken, since it is by neglecting them that the worst epidemics are caused and the milder ones aggravated and protracted. In other words, by avoiding contagion we shall prevent epidemics of very bad ophthalmia, and shall to a con-

siderable, although incomplete, extent avoid the permanent establishment in schools of the slighter forms of acute conjunctivitis.

In view of the fact that a very large proportion of acute conjunctival attacks are relapses, *i. e.* occur in those who have suffered in the same way before and are likely to suffer again, I think the first object should be to separate all children whose lids are diseased and who are known to have had acute attacks, or who have chronic conjunctival discharge, from the rest of the school. The ordinary plan of keeping these children among the healthy ones between their attacks and then sending them to the infirmary for a short time till they are cured of the acute symptoms, is miserably inefficient as regards the cure of the disease, is very hurtful to the children so dealt with, and allows them to be a constant source of risk to the healthy ones. The only practicable way of preventing these cases from acting as possible sources of contagion to healthy children is to keep them in an entirely separate building for several years until they have ceased for a year or two to have any relapses, and in order to be complete this must include separate arrangements for teaching and for play.[1] There is no reason why the buildings need be far removed from one another, although practically, no doubt it will often be best to have them on totally distinct premises.

The important object of avoiding contagion should be kept in view in all the details of such an establishment, but must not be allowed to divert attention too much from other preventible causes of acute ophthalmia. The most important details are—first, the prohibition of basins for washing (except a few for the infants, to be used only by a nurse), and the provision in their stead of the jet system of washing, by which if properly carried out, it is impossible for the same water to be used by two children or for any particles of discharge to adhere to surfaces from which they could be conveyed to the eye; secondly, a very frequent supply of clean towels, or else the provision of a separate towel for each child with arrangements for ensuring that each one always uses the same towel. On the whole I think that as good a result is obtained by the provision at each washing (*i. e.* two or three times daily) of a new and sufficient batch of large round towels, as by distributing a separate towel to each child at each washing, or by elaborate arrangements for keeping each child to the use of the same unchanged towel for several days. It must be borne in mind that when under treatment most of the children will have but little discharge and that the risk of contagion among each other will, therefore, be very small. A provision should be made, however, by which the eyelids of those who

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[1] This plan was strongly recommended to the authorities of one school by Mr. Critchett several years ago, and is again advocated by Dr. Bridges in his recent 'Report on Ophthalmia' to the Local Government Board.

have much discharge in the morning should be cleaned carefully before being allowed to wash.[1] Those who have much discharge in the daytime will of course whilst acute, be warded, and as to the few who constantly have a good deal of chronic discharge it is easy to arrange that they shall have their eyes cleaned separately several times a day. The pocket-handkerchiefs ought to be sewn on to the childrens' dresses, and should be shown to an officer several times a day so as to ensure their not being torn off. Of course no bed should contain more than one child. In respect to bathing, whether this be in separate small baths or in a single large bath, it will be easy to provide, in a properly arranged institution, that the cases with much discharge shall bathe after the other children, and all should be made to wash their faces separately just before bathing. I should not be inclined to adopt any measures of disinfection (except in an institution where there had lately been an epidemic of purulent ophthalmia) beyond ordinary cleanliness of rooms, furniture, and utensils. Nor would I attempt general disinfection of linen or other clothing or of bedding, except under the same unusual circumstances, and perhaps also in special very bad cases if such should occur.

Much has been said at different times about the necessity of classifying cases according to their degree of severity in the wards and day-rooms, &c. I should think it undesirable that any case of severe purulent ophthalmia should sleep or live in the same room with others not similarly affected, and much more that several such cases should be mixed with comparatively healthy children. It is further desirable that children who are known to be subject to severe relapses should sleep apart from those who are in a more stable condition of cure; the same dormitories might also conveniently include cases with considerable chronic discharge. Beyond this it is not, however, practicable to go, and even to the extent here indicated it will not always be found easy to attain in a school or infirmary with large wards and comparatively few officers, and which is constantly nearly full of children.

There are other reasons besides the prevention of contagion which render the permanent separation of ophthalmic children from healthy ones necessary. The foremost of these, which has been mentioned above, is that the education (using the word in its largest sense) of the ophthalmic children may not be interrupted more than is absolutely necessary while they are suffering or are liable to suffer from the disease. Again, more or less special provisions are desirable for some of them, *e.g.* those with opacities of the cornea or with

[1] An excellent plan, which is carried out by Dr. Duke at Anerley, is to douche each child's face and eyelids with a stream from a fine rose-jet, and then clean off the moistened discharge with bits of tow which are afterwards burnt.

persistent photophobia, and for others whose general health is bad, for it often happens that ophthalmia attacks most severely those who are least able to resist external causes of disease. To condemn all these children to several years of idleness, or what is almost as bad, to years of intermittent schooling and idleness, is to do them a great injustice, and to add to their already somewhat impaired prospects of earning an early and good living. The bad effect of the ordinary plan on these children must be seen to be appreciated.

Those cases which are not treated personally by the medical officer, or in which all treatment has been omitted for a time, should be inspected by him at moderately short intervals, say once a week.

The playground should, if possible, be of grass, and in default of this either asphalte[1] or flags should be used.

Such an ophthalmic school as is here intended should have wards set apart for its own ordinary sick cases, so that these need not be mixed with the sick children from the rest of the school.

All ophthalmic cases in which the state of the eyes admits of it should have as much out-of-door exercise as possible, and this may be increased in good weather by out-of-door school. I believe recovery is much retarded by the bad habit, which is not uncommon in these schools and out of them, of keeping all cases of eye-disease in the house and even in darkened rooms, under the impression that light and air are bad for the eyes. As a rule, to which there are certain tolerably well-marked exceptions, both light and air are highly beneficial to patients with granular ophthalmia and its complications, and the exceptions may be reduced to a minimum by the free use of large shades for photophobic cases, and by careful observation of the weather and adjustment of out-door exercise in susceptible cases to its changes.

It is probable that early and slight granular disease will be common among the so-called healthy children who form the main body of such a school and these must be expected to furnish from time to time a few new cases of ophthalmia requiring treatment. The whole school should therefore be inspected at regular intervals by the medical officer with this fact in view. I think that one inspection a week ought to be enough, provided that the other officials are instructed to separate any cases in which there is discharge or redness of the eyeball between the inspections. Every now and then an inspection should be made by the medical officer early in the morning while the children are in bed, and this should be more frequent at times when ophthalmia is known, or expected, to be temporarily on the increase, *e. g.* during prevalence of cold winds and of unusually dusty weather and after excursions or long walks.

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[1] Asphalte and tar-paving are more dusty, and dirtier than flags.

It will be safest that the arrangements for washing and for pocket-handkerchiefs should be the same as those in the Ophthalmic School, and the playground should, if possible, be a grass field.

Strict measures must be enforced not to admit from outside, among the healthy children, cases of conjunctivitis or of bad granular lids.

With all these precautions fresh cases will arise now and then. These will be more numerous in some years and at some seasons than at others, but they ought always to be detected and separated at an early stage.

Practical Conclusions.

1. Ophthalmia, such as at present is common in some schools, and still exists to a slight extent in the army, is a compound disease consisting primarily of chronic granular disease of the conjunctiva, to which there may or may not be added an acute conjunctivitis. It also includes cases complicated with ophthalmia tarsi.

2. Precisely the same combinations of conjunctival diseases are common among the poor classes of this and other countries as are found in schools for the children of such people, and in the ranks of the army.

3. Granular ophthalmia had existed for an indefinitely long time in many European countries before the date of the Egyptian campaign.

4. Granular ophthalmia is not produced by contagion or infection. It is caused probably by the action of moist air rendered impure by animal matter (not organized particles). A moderately high temperature seems also to be necessary or very favorable to its development.

5. The cause acts very slowly, a continuous exposure to it of nearly twelve months being probably necessary in order to produce an early stage. It continues to act after the early stage has been formed, and the disease thus becomes worse in proportion to the length of exposure. The severity of the disease also varies directly with the *intensity* of the cause.

6. It is probably impossible in a damp country where the dwellings are much overcrowded to prevent a considerable number of the poorer classes from being affected with slight degrees of granular ophthalmia; but its extensive prevalence in a severe degree shows a culpable neglect of hygienic rules among the body of persons so affected.

7. Granular ophthalmia is the result of a slow local tissue-change, and the tendency to it becomes in time hereditary. It undergoes spontaneous cure after several years, but the conjunctiva does not regain its normal structure or appearance.

8. Many lower animals (mammals and birds) are subject to the same disease.

9. A conjunctiva affected by this disease, while new or progressive, is much less able to resist the causes of acute conjunctivitis than a healthy one; many influences which produce little or no effect on a healthy conjunctiva thus becomes of importance as causing acute ophthalmia in granulous persons. Such attacks are more prolonged and often more severe than in healthy persons; they tend to increase permanently the degree of the granular disease; they leave behind a more unstable condition of the conjunctiva than preceded them and thus a great liability to relapse.

10. The subjective symptoms and tendency to relapse accompanying granular lids do not correspond in degree with the structural changes in different cases; but both this relation, and the character of the relapses, usually remain constant in each case. The same treatment does not always produce a similar amount of improvement in different cases, but its effect at different times on the same case is generally constant.

11. When the disease has existed a long time without the occurrence of acute conjunctivitis this increased susceptibility to inflammation gradually lessens, and at length ceases; the prospect of a severe attack of ophthalmia, therefore, becomes smaller in proportion to the length of time that has already elapsed without one since the granular condition began. It is necessary to inspect such cases repeatedly for many months before it can be affirmed with any safety that they have reached this stable condition.

12. Muco-purulent or purulent discharge from the conjunctiva, arising from any cause whatever, is contagious. It will reproduce conjunctivitis but cannot cause granular ophthalmia. The severity of the reproduced disease depends partly on the character of the *contagium*, partly on the condition of the recipient's conjunctiva, and to some extent also on his state of general health.

13. Conjunctivitis of every possible degree is produced by many other causes than contagion. It may assume its severest forms alike in healthy and in granular lids.

14. Except under very bad conditions many of the cases of mild ophthalmia which occur in granulous persons are due to other causes than contagion. Precautions against contagion, while preventing extensive outbreaks of both mild and purulent ophthalmia, will not insure complete immunity from either.

15. Most of the constant causes of conjunctivitis in granulous persons, *e.g.* draughts, damp, dust, measles, are at least partially preventible; but various occasional ones are not so, *e.g.* epidemic influenza and causes depending on climate.

16. Consequently wherever granular ophthalmia is allowed to be very prevalent and attain any severity of degree, attacks of acute

conjunctivitis and serious sequelæ of ophthalmia, will be common, unless great precautions be taken to prevent the introduction of contagious cases and the operation of the other constant causes.

17. It is therefore of great importance to diminish the prevalence and the degree of granular ophthalmia, especially in large institutions.

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II.—On the Physical Requirements of the Soldier. By A. Leith Adams, M.B., F.R.S., Surgeon-Major Army Medical Department.

The following data referring to the physique of the soldier were for the most part collected during three years' official inspection of recruits in the largest depôt in the United Kingdom.

The points I have sought to demonstrate and the objects I have aimed at elucidating must be allowed to be of vast importance to the army surgeon, seeing that of late years many vexed questions have arisen with reference to the physical capabilities of recruits.

Put in the form of a question, the subjects of inquiry resolve themselves into the following:

What are the proper standards of height, weight, and chest capacity, at the various ages at which boys and recruits are eligible for military service?

Although by way of comparison I have attempted to correlate the military with the civil population, it must be understood that the figures relating to the former have no pretence to be considered in any way as referring to the extremes or averages of the population generally. They are purely of a medico-military character, having been selected by me from a heterogeneous assemblage of persons who offered themselves for military service. The data are therefore furnished with the intention of demonstrating what requirements of height, weight, and chest capacity appear to me good exponents, not only of the age of the soldier on enlistment, but also with reference to what he might be expected to attain to at manhood.

From the following table several important deductions may be drawn in connection with the four physical requirements—age, height, weight, and chest girth. These I shall now proceed to consider from a medico-military point of view.

I. Age.

In armies maintained by a voluntary system of enlistment, and more especially should there be a dearth of recruits of matured years, it will always be the case that, as the supplies decrease, the greater will be the risks of trenching on physical inefficiency.

If, as in the British army, the ages are embraced between eighteen and twenty-five, we will be sure to find a very large percentage of lads of seventeen asserting that they are eighteen or over, and men

Table showing the relative height, weight, and chest circumference at various ages.

Sources of information and numbers inspected.	Age.	Height.			Weight.			Chest girth over the scapulæ.			Chest girth below scapulæ after deep inspiration.			Chest girth below scapulæ after deep expiration.			Difference between inspiration and expiration.		
		Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.
		in.	in.	in.	lbs.	lbs.	lbs.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
London Recruiting District (37)[1] .	14 to 15	61	52	55·7	96	65	77·5	31	26	27·8	31·5	27	28·8	29	25	23·5	4	1	2·6
Royal Military Asylum, Chelsea (771)[2]	14 to 15	64	49	55·5	115·5	49	73·9	30·5	23·5	27									
Gymnasium, Oxford, Maclaren (25)	14	66·4	56	61·4	125	81	98·4	33	27	28·4									
Some boys after one year's training (25)	15	69·5	57·5	63·5	132	82	106·5	36½	27·7	31·4									
Harrison's Tables (197) . .	14 to 15	63	48	55	...	...	...	32	21	26·5									
London Recruiting District (28) .	17 to 18	69	63·5	66	143	115	126·3	36	33	34·5	36	32·5	34	32	29	29·6	4	2	2·9
Harrison's Tables (151) . .	17 to 18	67	51	60·7	...	...	...	34	23	27·5									
London Recruiting District (109) .	18 to 19	71	64·5	66·3	152	118	135·3	38	33·5	36	38	33	36·3	36	30	32·4	4·5	1·5	2·3
Danson's Tables (100 and upwards)	18 to 19	71	58·5	64·34[3]	153	90	122·79												
London Recruiting District (80) .	19 to 20	72·5	64·5	67·1	170	116	135·3	38	33	35·4	38	30	35	35	30	32·4	4·5	1	2·8
Danson's Tables (100 and upwards)	19 to 20	71·5	59	64·94	176	102	130·11												
London Recruiting District (49) .	20 to 21	72	64·4	67·6	180	123	143·4	40	34	35·1	39·5	33	36·5	36	31	33·4	45	1·5	2·7
Danson's Tables (100 and upwards)	20 to 21	71	61	65·11	176	111	135·58												
London Recruiting District (47) .	21 to 22	72	64·5	67·4	180	125	144·5	41	34	36·2	39·5	34	36·4	37	31	33·4	5	1·5	2·9
Danson's Tables (100 and upwards)	21 to 22	71·4	60·5	65·57	168	101	131·02												

London Recruiting District (25) .	22 to 23	71	65	67·7	160	130	144·6	39	34·5	36·1	39	34·5	36·8	36[4]	32·1	33·6	3·5	1	2·4
Danson's Tables (100 and upwards)	22 to 23	73	60·4	66·17	184	98	138·41												
London Recruiting District (17) .	23 to 24	72·7	65·5	67·6	182	119	141·2	39	34	35·9	38	34	36	36·5	30	32·9	4	1·5	3
Danson's Tables (100 and upwards)	23 to 24	73	59	66·17	180	110	142·95												
London Recruiting District (23) .	24 to 25	72·5	65	67·3	199	122	144·9	40	35	36·1	39	34	36·2	36	31·5	33·7	4·5	1	2·4
Danson's Tables (100 and upwards)	24 to 25	73	57	65·9	180	110	142												
London Recruiting District (9) .	25 to 30	69	65·5	67·1	188	124	141	40	34	36·1	41	34	36·2	38	32	33·5	3·5	2	2·7
Danson's Tables (100 and upwards)	25 to 30	72	59	66·30	190	114	145·65												

1 The data here are exclusive of the measurements of the Chelsea boys, who go through the London Recruiting Office before entering the army.

2 Thus, since 1860, of all the boys admitted into the asylum the tallest at fourteen years of age was 64 in. in height and weighed 115½ lbs.; the shortest being only 49 in. in height, and weight 49 lbs.

3 Quetelet, in his work 'Sur l'Homme,' makes the average height in Belgium at this age 1·658 mètres, or 5 ft. 5·27 in. According to the data furnished by him, growth is nearly completed at eighteen, as he gives only an inch afterwards, an assertion not maintainable as regards our race; and although the average here given by Dawson is probably below, it may be accepted that ordinarily two inches are added to the height after seventeen in men whose stature is sixty-seven or sixty-eight inches at thirty.

4 The average age of the recruits for the City of London Police Force, one of the most magnificent body of men to be seen in any service, is twenty-two and a half years. Of 300 recruits for the year 1872 I am informed by the surgeon, Mr. Childs, that the average height is 5 ft. 9 in. and chest girth 37⅛ in.

of thirty to even forty ready to swear that they are under twenty-five years of age.

The difficulty in arriving at anything like a correct judgment is far greater at the lowest limit. It would be less so at seventeen, for the difference between sixteen and seventeen as regards physique is generally more easily determined than between the latter and eighteen; besides, provided seventeen was made the minimum military age, certain requirements as to height and weight might (as will be suggested presently in the case of boys) be demanded as a safeguard.

The grand point for the surgeon to consider at the maximum limit is whether or not there are the promises of sufficient stamina in the man for the shortest period for which he could be enlisted, without the risks of his breaking down or becoming a burden to the service. For instance, many men to all appearance thirty years of age show indubitable promises of at least twelve years' good work. The only drawback to these recruits, particularly in the case of navvies and agricultural labourers, is that they lack the activity and aptness of younger men, although they rival them in strength and "staying power."

Of course careful selection is necessary at both extremes of age. Thus, especial selection of men from the present maximum limit up to thirty might be made with seeming advantage, at all events for short periods of service.

Boys.

Boys are enlisted as musicians, drummers, buglers, or tailors, between fourteen and sixteen years of age. No physical requirements are demanded, as in the Navy, so that there is no rule to guide the military authorities; neither are birth certificates furnished, as in the former and in the case of the boys of the Royal Military Schools at Chelsea and Dublin. In the London district alone, during the last three years ending December, 1873, no less than 500 were passed into the service from various charity schools and the Goliath training ship.

The object, of course, in public charities of every sort is to get rid of the boys whenever they arrive at the eligible age. But seeing that no register of birth is preserved, it is left to the authorities to estimate the probable age of the lad, so that he may be furnished with an occupation as soon as possible. Again, very many of the boys have lost one or both parents, whilst all more or less partake of the physical aspect of the town-bred lad. Their aptitude for music and tailoring being the military requirements, any intelligent, puny, and consequently not unfrequently precocious child who displays a turn one way or the other is selected for the army, whilst the larger boys take to rougher occupations, and are readily sought

after by tradesmen as apprentices and so forth. From the above it is evident that none but well-developed lads of the average height and weight, of boys of fourteen years of age, should be taken; and even allowing for the diversities in these respects of lads in their teens, it will be conceded, in the case of the charity schools of London and other large cities, that a standard of physical requirements might be established with advantage.

The lowest limits in the navy is fifteen years, unless in special instances, where boys between fourteen and a half and fifteen are received provided they are $56\frac{1}{2}$ inches in height and not less than $27\frac{1}{2}$ inches round the chest.

Referring to the table—

I. I am indebted to Mr. Crosse, of the Royal Military Asylum, Chelsea, for the data shown as to the boys under his care. It must be stated, as regards height and chest girth, that these boys had been drilled and trained, many for seven years; moreover, being born of one parent of military stature is in their favour with reference to the latter.

II. The calculations Mr. Maclaren has been good enough to permit me to publish were taken from young gentlemen who attended the Oxford gymnasium. As compared with the other admeasurements in the table, they very much exceed any data to which I have had access, but nothing beyond what might be expected of lads who had been carefully reared from infancy. The measurements are further suggestive as showing the marvellous effects of well-directed gymnastic exercises on youths, and how much such would benefit boys enlisted at fourteen, who are not at present amenable to a course of athletic training.

III. Harrison's tables[1] were formed haphazard from the data furnished by factory children in a crowded city, and include both sexes, hence the chest girth is valueless beyond the age of fourteen years.

IV. It must be fully understood that the boys examined by me were selected from a large collection of the usual run of lads brought up for inspection. The same to some extent obtains with the soldiers' sons at the Royal Asylums.

Some idea of the inefficiency represented in the case of the former may be gathered from the circumstance that, out of 673 boys brought before me from charity schools, no less than 167 were found unfit on account of muscular tenuity and stunted growth, and even of the number accepted not a few appeared to be one or two years younger than stated. The military surgeon is therefore helpless in such cases, unless he establishes standards of height, weight, and chest girth. These, of course, will vary in individual

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[1] 'Edinburgh Medical and Surgical Journal,' 1835, p. 425.

cases; still, within certain limits, it is clear that a well-nourished promising boy of fourteen years of age should be 4 feet 7 inches in height, weight 74 pounds, and chest girth 27 inches, or by the infra-scapular method to be detailed presently the maximum girth expansion should be 28 to 29 inches, and the girth after an expiration 26 to 27 inches. This I found to be the only safe way of dealing with the heterogeneous collections of London boys in particular.

The military method of taking the chest circumferences as in older persons is extremely fallacious and unreliable for exact medical or other evidence. Healthy boys whose ages are determinable might be safely accepted an inch to two inches under the above height, but in such cases the weight ought not to be under 73 pounds.

Although the boy age extends to sixteen, very few are obtained over fourteen, for the reason that the latter is the limit of maintenance at the various schools from whence boys are furnished. I have consequently been unable to obtain data in connection with boys between fifteen and sixteen years age.

"Eligible lads between seventeen and eighteen years of age."[1]

Under special circumstances, very promising lads of seventeen and short of eighteen can be enlisted on the certificate of the examining surgeon that they "are in all respects fit for the service." The latter virtually means that they are very promising lads: to perform the ordinary duties of a soldier, in peace or war, being impossible at this tender age. At all events, whenever anything like hard work is imposed, the advantage has always (as may be supposed) been with older men.

In the table I have correlated what appears to me the maximum and minimum measurements of eligible lads between seventeen and eighteen; and taking into consideration what are designated "race characters"—in other words, that the Anglo-Saxon is physically not of the shorter Celtic stature—these data appear to me fair exponents of what we should expect at seventeen years of age. Of course there are qualifying exceptions, but in view of future requirements I think military youths should show a minimum height of 5 feet 5 inches, weight 120 pounds, a chest girth over the shoulder-blades of 34 inches, and a maximum expansion and minimum contraction of the chest of 33 and 30 to 31 inches.

It is noteworthy that the scapulæ project more in flat-chested and rapidly growing lads than in matured men, hence they increase the girth to a deceitful extent; indeed, a comparison of the two

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[1] See General Order, No. 95, November 1st, 1872.

methods in the table shows how valueless is the supra-scapular admeasurement as an index of thoracic capacity.

The averages shown in the table, viz. height 66 inches, weight 126 pounds, chest girth over shoulder-blade $34\frac{1}{2}$ inches, with a chest expansion of 34, and a minimum chest contraction of 31 to 32, were obtained from 28 youths especially selected as good representatives of the age under consideration. These selected youths, compared with the 151 factory boys and girls taken from the general population, give remarkable contrasts. As to chest, as just observed, no reliance is to be placed on Mr. Harrison's measurements for obvious sexual reasons.

Between eighteen and nineteen years of age.

This being the minimum recruiting age for the able-bodied soldier, great deception is practised. Taking the numbers of men who do not know their ages exactly, and the greater portion who purposely make misstatements, there is very frequently nothing left to the surgeon and the military officer but the exercise of their own judgments in determining whether or not the youth is capable of bearing arms.

If the lad's assertion is invariably accepted, and the military authorities adhere strictly to the letter of the law regarding requirements of height and chest girth, whilst the surgeon merely pronounces on the soundness of his health, then there may be no end to the admission into the service of young and feeble youths and men of poor physique. The above age, like the preceding, being only that of the promising youth, demands simply an advance on the physique, but such an improvement over seventeen that, looking towards twenty, it is necessary to exact certain data in relation to height, weight, and chest, and these must be considered with reference to the demands of different arms of the service. Thus, the 60th Regiment Rifle Brigade and artillery drivers take youths of this age as low as 5 ft. $4\frac{1}{2}$ in. in height, whilst tall powerful men are wanted for gunners and heavy cavalry; again, the light dragoon and lancer, and finally the linesman and engineer, all of whom demand from the nature of their work the possession of excellent stamina. It has, however, been the case of late years that the infantry service has failed to attract so many able-bodied men as formerly, whilst the cavalry ordinarily continue to receive supplies of much the same quality as heretofore.

I. The *beau idéal* of an active skirmisher will be found assuredly among the short dapper men of the two regiments with dark uniform —"cobby little fellows," as a colonel of one of these rifle corps expressed himself when giving directions to my coadjutor Colonel Lyons regarding recruits for his battalion. All other points being

equal, that is, taking into consideration the set appearance of the lad of eighteen, I would expect him at that age to stand not under 5 ft. 3½ in. in height, weight 126 lbs., chest over scapulæ 35 in., greatest expansion 34 in., and minimum contraction 32 in. The great danger of introducing inefficiency at this age with the above height, or even 5 ft. 4½ in., will arise from want of attention to the general physical characters which experience has shown are adaptable to the work of the small-sized rifleman and driver of artillery; indeed, this caution is applicable to all ages and to all branches of combatants. At headquarters of corps it is looked to, but at recruiting districts it has been the habit to stick too much to regulation as regards height and chest girth, not taking probable or real age in any ways into account. This, considered medically, is a serious physiological error, to which I will again revert in the sequel.

II. The second taller type of man for the general infantry service should at eighteen years of age give a minimum height of 5 ft. 5. in., weight from 125 to 130 lbs., and chest girth over scapulæ of 34 in., and a maximum expansion of 34½ in., and a minimum of about 31½ to 32 in. Danson's tables are valuable as far as they extend, but only the height and weight are given. The records were taken from 4800 British male criminals, and the averages were derived from 100 observations and upwards at each age.[1] According to this authority the mean height was 64·3 in. and weight 122·79 lbs., which are not far below what I have advanced for the soldier at this age. Dr. Aitken,[2] arguing from Danson's statistics and other data, gives an average height of 63·003 inches to the lad of eighteen years of age, a figure which, however applicable to the general mass of the population, would be sure to produce evil results if acted on as a standard of military stature in the Anglo-Saxon.

There is no especial object in continuing comparisons or furnishing data beyond nineteen up to twenty-five years of age, as, provided the requirements of youths up to the former are clearly realised, there can be no difficulty in adjudicating the standard afterwards. In fact, the whole difficulty is to be assured that the promising lads of seventeen and eighteen carry sufficient physical characters so as to ensure that they will turn out to be able-bodied men of 67 in. in height at twenty-five years of age. Nothing, of course, is more variable than the rate of growth; it may, however, be accepted that a well-nourished healthy lad, who at manhood should attain to the above height, will have to make at least two inches between eighteen and twenty-five. Very little is added after twenty, whilst the majority of lads make the greatest strides in height between fourteen and that age. Thus, I have always been of opinion that the recruit of

[1] The difficulty in being certain of the age of the inmate of a gaol must, if anything, be rather greater than in the case of the recruit.

[2] 'Science and Practice of Medicine,' vol. ii, p. 216.

64½ to 65 in. at twenty is not likely to add even an inch subsequently to his height; in fact, the 60th Rifles and Rifle Brigade and artillery drivers, in order to maintain uniformity of their low stature, should be recruited by men not under twenty years of age.

The statistics of the armies and peoples of Europe furnish instructive illustrations of diversities in stature. Thus, Pouchet[1] asserts that the inhabitants of the south of France are demonstrably shorter than Frenchmen of the northern departments. This statement is further shown to have some intrinsic value, whether as "a race character" or a sign of "deterioration of race," by the rejection of no less than 10 per cent. per 1000 of conscripts examined between 1836 and 1840. Moreover, Danson shows in his excellent paper referred to that the French army from 1835 to 1843 gave a common average of height of 65½ in. There is one thing always to be borne in mind with reference to the physique of the French soldier, viz. that since the beginning of the century there has been a terrible drain on the population for warlike purposes. It must, therefore, be a matter for speculation how much any decrease is owing to the baneful results of wholesale conscriptions carried on for years or to inherited race characters, or both. No doubt the ancient Celt was of small stature as compared with the Teuton and Anglo-Saxon of the present day, but since those times there has been such an intermixture of races that to attempt mathematical accuracy would be useless.

Published data in connection with the Austrian conscription of 1870 shows that of men of the classes of 1848, 1849, and 1850, no less than 141 per 1000 were rejected as being under the minimum height standard of 61·19 in., and 565 for medical reasons. The differences in stature between conscripts raised in the northern and southern commands are remarkable. Thus, the stature of the former varied from 5 ft. 4 in. to a little over 5 ft. 5 in., with an average chest girth (taken between the respiratory acts) between 32 and a little over 33 in. Poles, moreover, were still shorter, the height varying from 63·256 to 64·25 in., whilst the Croats averaged 65·4 in., and the Dalmatians, the tallest of all, gave an average between 66·4 and 67·7 in.[2]

It is well known that men at any given age attain the same average height and weight as older or younger men; consequently, regarded from a military standpoint, our great aim should be to discriminate as closely as possible all varieties of stature and weight at the uncertain ages between seventeen and nineteen, seeing that beyond the latter there is never any difficulty in estimating what the man should be.

The extension of the army age to thirty would bring a certain

[1] 'Plurality of Races,' p. 47.

[2] I am indebted to my friend Captain Cooke, Statistical Department, War Office, for these figures.

increase, but selection would be specially necessitated, and the recruit could only be engaged for short service, so that, including the reserve service, he would be forty-two years of age on his discharge from the latter. In order to give scope for selection the limits of age, therefore, might be seventeen and thirty.

II. Height.

Standard requirements.

According to existing regulations, the following are the physical requirements of the various branches of the service, showing the chest girth to height.

	Age.	Chest to Height.		
		in. ft. in. ft. in.	in. ft. in. ft. in.	in. ft. in.
Infantry . .	18 to 25	33 fr. 5 5 to 5 8	: 34 fr. 5 8 to 5 10	: 35 fr. 5 10 and upw.
60th Regiment Rifle Brigade .	18 „ 25	34 „ 5 4½ „ 5 7		
Drivers R.A. .	19 „ 25	35 „ 5 4½ „ 5 6½		
Gunners R.A. .	18 „ 25	33 „ 5 7 „ 5 8	: 34 „ 5 8 „ 5 10	: 35 „ 5 10 „
Heavy Cavalry .	18 „ 25	34 „ 5 8 „ 5 10	: 35 „ 5 10 „ 5 11	
Medium ditto .	18 „ 25	34 „ 5 7 „ 5 9		
Hussar & Lancer	18 „ 25	33 „ 5 6 „ 5 8	: 34 „ 5 8 „ 5 10	: 35 „ 5 10 „

One of the most noteworthy particulars in the above is the lowest chest girth required for infantry, gunners, and cavalry, between eighteen and twenty-five years of age, and between 5 ft. 5 in. and 5 ft. 8 in. Thus, a circumference of 33 in. chest at twenty-five years of age in a man 5 ft. 8 in. in height represents a very slight frame. Again, a man of the latter height and eighteen years of age must be looked on with grave suspicion as to his future physical capabilities; both conditions may be said to be beyond the border line of natural growth, at all events in a vast majority of instances. The surgeon, indeed, who accepts these figures and merely gives the recruit what may be called a "clean bill of health," would most assuredly be lending his hand at introducing serious elements of inefficiency into the army. In fine, chest girth is only useful to the recruiter if he chooses to employ it in making choice of the volunteer, so as to save himself the trouble and expense of bringing up weeds and boys. To be even of effectual use to him the minimum chest girth should not be under 34 in. Next to height, of course, weight is the most important factor in determining the physical capabilities of the recruit, but unfortunately it is the least reliable, as will appear presently.

Lowest standard of height and ranges of age in Continental armies.

	Height.	Age.	
Sweeden . .	65·5	17—30	Mostly voluntary enlistment.
British . .	65·	18—25	Reduced to 64½ and 64 inches on urgent occasions.
German . .	63·7	20	In special cases and during urgency the standard is lowered to 61·826.
Danish . .	62·83	21	The liability to serve commences at 17, but enrolment does not take place until 20.
Austrian . .	61·19	20	
Norwegian .	61·10	22	
Belgian . .	?	19	
Italian . .	?	21	
Russian . .	?	21	
French . .	60·3	20	
Roman soldier .	62·5	...	Liability to serve began at 17.

It will be observed that the advantages as regards height are with the voluntary systems, which, again, have the disadvantage on the score of age. Thus, whatever the lad in his teens may display as compared with the shortest conscript, the latter is far more likely to withstand sudden and severe prolonged exertion, and of course, all other points being equal, he is the better suited for warfare. Again, in order to make the above figures at all useful in determining the physique of the fighting man, it is necessary to know what proportion in numbers men of the minimum standard of height bear to taller conscripts, and also the other physical requirements which should accompany them.

It would seem, however, that the lower the minimum military standard of a nation, that either the stature is naturally short or else there is a difficulty in meeting demands by taller men. Our soldiers, especially the infantry soldier, is of less stature now than formerly. Of 351 pensioners of the line at present in the Royal Hospital, Chelsea, Dr. Legertwood informs me that the average height is 5 feet 7½ inches. These veterans, borne down by the weight of years and infirmities, and taken promiscuously from regiments, furnish a fair exponent of the soldier of a quarter of a century ago, when the minimum stature of the recruit was fixed at 5 feet 6 inches.

Of the conscriptive armies, the Germans naturally take the first place with reference to stature.

It must be borne well in mind, however, that with many short races there is a relative development of chest capacity, weight, and stamina, so that they may be as physically equal to the *ordinary*

requirements of warfare as a much larger people; nevertheless, how long an army composed of short men would withstand the impetus, if I may so use the expression, of taller and heavier men, not only physically but morally, is doubtful. In the struggle for existence among lower animals we find nature's law pretty constantly maintained; so with mankind, when short are confronted in battle with larger men, both equally bold and adventurous, the odds naturally will be against the former. But there are exceptions wherever the moral qualities are defective; thus, the stunted Ghoorka of Nepaul, a valorous mountaineer, is immeasurably superior in valour to the Hindostanee sepoy, although, physically, the latter is both much taller and stronger.

III. Weight.

For practical purposes it is unnecessary to seek a closer determination of weight than in pounds. All my observations, as in the case of height and chest girth, were taken when the man was perfectly naked. The above most important element in physique is of course the least reliable; indeed, it is more the exception than the rule to meet with an instance of a recruit being up to his highest standard of weight, and this is the case more especially among town-bred than country recruits. The reasons are obvious when we come to sift the recruit's history and the causes of enlistment in very many cases. Great allowances require, therefore, to be made for previous circumstances; at the same time it is requisite not to allow too broad a margin, without fully weighing every consideration in relation to the man's physical capacity, more especially the size of his bones and chest expansion.

The data furnished in the table represent the variations in weight of *selected* recruits at all ages. The lowest weights shown at the ages of eighteen and nineteen must invariably carry with them compensating conditions with reference to bodily development and clear indications of a promising increase; indeed, the man of 8 stone 4 pounds, or 8 stone 6 pounds, at any effective military age or height, has to add at least half a stone before he can attain to anything like the development of an able-bodied soldier, but the gain is often remarkable; however, in many instances the assurances at enlistment are poorly fulfilled afterwards, in spite of the advantages of excellent food, regular habits, and the salutary exercises of the gymnasium, and this is most generally the case with the city-bred youths, who have of late years taken the places of a vast number of countrymen. Those recruits may be active and more intelligent than the labouring man, but for campaigning purposes and ability to withstand trying climates they are clearly unsuited. Again, it is affirmed that the great revolutions in gunnery of late years seem to point more to the activity than the strength of the combatant,

and there may be some truth in the assertion, although it is difficult to separate the two conditions and think of the rapid and trying marches made by the Germans during the late war, also the modern systems of drill; besides, the English of all other soldiers should be selected for endurance, at all events with reference to foreign service. We are naturally a tall race; that is, what would be called a fair stature in certain countries when applied to Englishmen would take in very many of the puny undergrowths of our densely populated cities, more especially when the standard of height is reduced to its minimum.

With reference to the boys from the Military School at Chelsea, I am informed by Mr. Crosse, that, looking to all the lads who have passed under his observation for many years, and from a mass of data, that he had estimated the average weight of a well-nourished healthy boy of fourteen years at 77 pounds to a height of 56 inches. The value of this opinion is enhanced by the circumstance that the ages of the lads were known exactly, and that they had been placed under excellent hygienic conditions for several years, therefore everything had been done to raise their physiques to their utmost capacities. These data nearly agree with the averages I have deduced from thirty-seven selected boys from civil establishments, as seen in the table. Mr. Crosse further considers that a boy between seven and eight years of age should, in order to come up to the above standard, show a weight of 44·24 pounds to a height close on 45·08 inches.

IV. Chest Circumference.

Modes of measuring the chest.

The method pursued in our army is to place a tape quite horizontally round the chest in a line with the upper part of the nipple, so as to include the shoulder-blades also. It is not to be drawn tight, and during the process the arms hang loosely, whilst the recruit counts up to ten slowly.

The inclusion of the scapulæ and large chest and back muscles render this plan barely what may be called an approximation to the real thoracic internal capacity. This is the case just as much among the spare and weakly as with the large and well developed; indeed, the shoulder-blades often protrude more in the former than in the latter. As a military requirement the chest girth was introduced of late years. Formerly, when recruits were plentiful, it was not taken into consideration, for the reason that the standard height was not so low, and selection could be made of the strong and able-bodied only; since then the physique of the recruit has decreased, and in order to prevent the acceptance of ineligible men it was deemed requisite to establish a check on the limits of inefficiency,

which, of course, increased with the dearth of supplies. As in the cases of age and height, the chest circumference is vouched for by the military anthorities.

With reference to its surgical value, there is no need of remark; indeed, as far as being a criterion of the breathing power or endurance of the individual, it may *per se* be said to be of little or no value.

The posture of the arms also makes a difference. By the sides and when extended horizontally the girth is much the same, but when raised perpendicularly there is a certain increase which is more pointed in persons accustomed, such as blacksmiths, to exercise the dorsal muscles. Of course a very great deal depends on the condition of the respiratory act at the time; for example, in the Austrian army the girth is taken "during the pause between the two motions," whilst in the Prussian army it is made "after a maximum inspiration."[1]

As far as the chest girth is of value as an exponent of the breathing capacity, I have long practised the method well known, viz.—A narrow tape divided into tenth parts of inches is carried round under the inferior angles of the scapulæ and over the nipples, the recruit standing at ease with his arms stretched out slightly under the horizontal, when with his mouth closed he takes a deep inspiration.

2ndly. When the arms are dangling loosely by the sides he is desired to make a deep expiration or a prolonged whistle, so as to empty the lungs. The difference between the two acts varies a good deal in individuals, and very much depends on the intelligence of the recruit; indeed, what between a voluntary and almost instinctive disposition to keep the thorax expanded it requires great care in execution, and therefore, as a substitute for the present method, it is entirely beyond the intelligence of the recruiting sergeant.

The spirometer is also liable to give false results from want of care and inaptitude on the part of the experimenter. Its application in the recruit room would be serviceable in doubtful cases of lung disease, and of all instruments I have seen the very delicate pocket spirometer invented by M. Casela is about the most perfect. The indication, however, of sound lungs will be best tested by a faithful record of the *normal* respiration in contradistinction to a *forced* expiration.

According to the existing army regulations, a chest girth of only 33 in. at all intermediate ages from eighteen to twenty-five, and from 65 in. up to 68 in. in height, is required. Beyond the latter up to 40 in. in height an additional inch is demanded, and 35 in. of a chest circumference for any increase afterwards. However much these

[1] Dr. Körber's 'Results of the Re-examination of the Marine Recruit in the Russian Service.'

data may assist the recruiter in making choice among the very varied material among which he picks and chooses, they deserve small attention at the hands of the surgeon; at all events he will do well to be ruled in no ways by any apparent claim these considerations may be made to exact.

What might be called the "play of the chest," *i.e.* the difference between the maximum and minimum infra-scapular method just described, varies very much individually, and at all the ages from fourteen to twenty-five, but ordinarily it is between two to three inches.

It will be seen from the table that the maximum inspiration rarely exceeds the supra-scapular girth beyond an inch. Thus, by substituting the former plan for the latter in the relative requirements of age to height as laid down in recruiting regulations,[1] the chest girth would stand thus:—1. Men between eighteen and twenty-five years of age and between 65 and 68 in. in height should show a maximum of $33\frac{3}{4}$ in. and a minimum chest girth of 31 in. 2. Between 68 and 70 in. in height the two girths should stand $\frac{34\frac{3}{4}}{32\frac{1}{2}}$. 3. Over 70 in. in height, $\frac{35\frac{3}{4}}{33\frac{1}{2}}$ in.

It appears, therefore, that ordinarily there is about an inch between the girth taken during a deep inspiration as compared with that by the method at present in use in the army; much, however, depends on the care and attention bestowed on the operation.

The large experience afforded by the London recruiting district has demonstrated that the minimum chest girth of 33 in. is far more frequently incompatible than otherwise with physical efficiency as far as able-bodied soldiers are concerned, whilst by comparison it has been found to be a fair girth standard of promising lads between sixteen and seventeen years of age. Finally, with reference to age, it is very apparent that a large number of recruits of the lowest standard of height either wilfully or through ignorance exaggerate their age, so that, provided they come up to the requirements as regards stature and chest girth, no exception is ordinarily taken to the age, even when the countenance and general aspect clearly belie their oaths. Hence, instead of able-bodied men they are merely promising lads, who, if carefully trained, will probably turn out to be able-bodied soldiers in time. But the most serious results of a low physical standard is the likelihood of admitting the staminaless undergrowth of crowded cities into the ranks.

According to existing regulations, the responsibility as regards the physical efficiency of the recruit is divided between the military and medical officers who may pass him into the army. Should

[1] General Order, December, 1870.

discrepancies arise connected with the recruit's age, height, or chest girth, and the regulated standard, it is the former who becomes answerable, whilst the surgeon is accountable only for the presence of disease of a disqualifying nature. Hence, should the military requirements be of a low standard as regards physique, the medical officer may merely pronounce on the absence of disability, and thus, what between the misstatements of the recruit as regards his age and the divided responsibilities, it may easily happen that men physically unfit for the arduous duties of the British soldier would be often accepted.

The obvious remedy for the evil would be to place the entire responsibility of the physique of the soldier in the hands of the surgeon, who demonstrably is the legitimate authority, and whose judgment, matured by experience of the man and his work, should be equal to the occasion. The objections to this mode of procedure are likewise patent. According to a voluntary system of enlistment, carried out on existing principles with a vicarious supply and an ever constant demand, it might so happen that the former would not meet the latter when the surgeon finds himself the sole arbitrator of the physical requirements of the recruit. But of one point I feel assured, viz. that it is far better that an army should be composed of able-bodied men than expanded by weeds and boys.

III.—Case of Capsulated Scirrhus of the Breast, with Remarks. By Charles J. Cullingworth, Surgeon to St. Mary's Hospital, Manchester.

The following case is offered as a contribution to the study of cancer of the breast, and as having an important bearing on the relation subsisting between that disease and simple lobular hypertrophy.

Elizabeth C—, æt. 56, married, and a resident in Manchester, was admitted into St. Mary's Hospital for Women on the 1st of June, 1874.

She had presented herself five days previously as an out-patient, on account of a tumour in the right breast, which had been first noticed seven years ago. She looked ill and emaciated, and she told me that she had occasionally vomited her food for some weeks, and during the past fortnight incessantly. The tumour was in size equal to a small fist, freely movable, and not adherent to the skin, which played smoothly over it and looked perfectly natural. The nipple was unretracted, and there were no enlarged glands to be felt in the axilla. I made a rough diagnosis of the tumour as non-malignant and suitable for enucleation, and accordingly directed her to come into the hospital, with a view of examining her more carefully, improving her general health, and eventually operating.

On admission I examined the abdomen, and found the walls thin, flaccid, and unresisting. In the lower part there were several hard lumps, of the size of walnuts, deeply seated; midway between the xiphoid cartilage and the umbilicus in the median line there was a rather larger mass, hard, irregular, nodulated, freely movable, and neither tender nor painful. Large enemata were ordered, and, through their agency, all the lumps disappeared except the larger one, which changed its position from day to day, one day being discovered just below the umbilicus, and, the next day, close to the xiphoid cartilage. No food of any kind could be retained by the stomach. Nutritive enemata were administered every four hours, but the patient rapidly sank, and, after a few days of muttering delirium, died on the 18th June, not quite three weeks after her admission.

There was found, on post-mortem examination, a mass of scirrhous cancer[1] at the pylorus, of the size of a walnut, with narrowing of the orifice to such an extent that water would only pass through slowly, drop by drop. No deposit was discovered in any other of the viscera. The mammary tumour was removed, and handed over for careful examination to my friend Mr. Thomas

[1] Since deposited, along with a section of the breast tumour, in the Hunterian Museum at the Royal College of Surgeons.

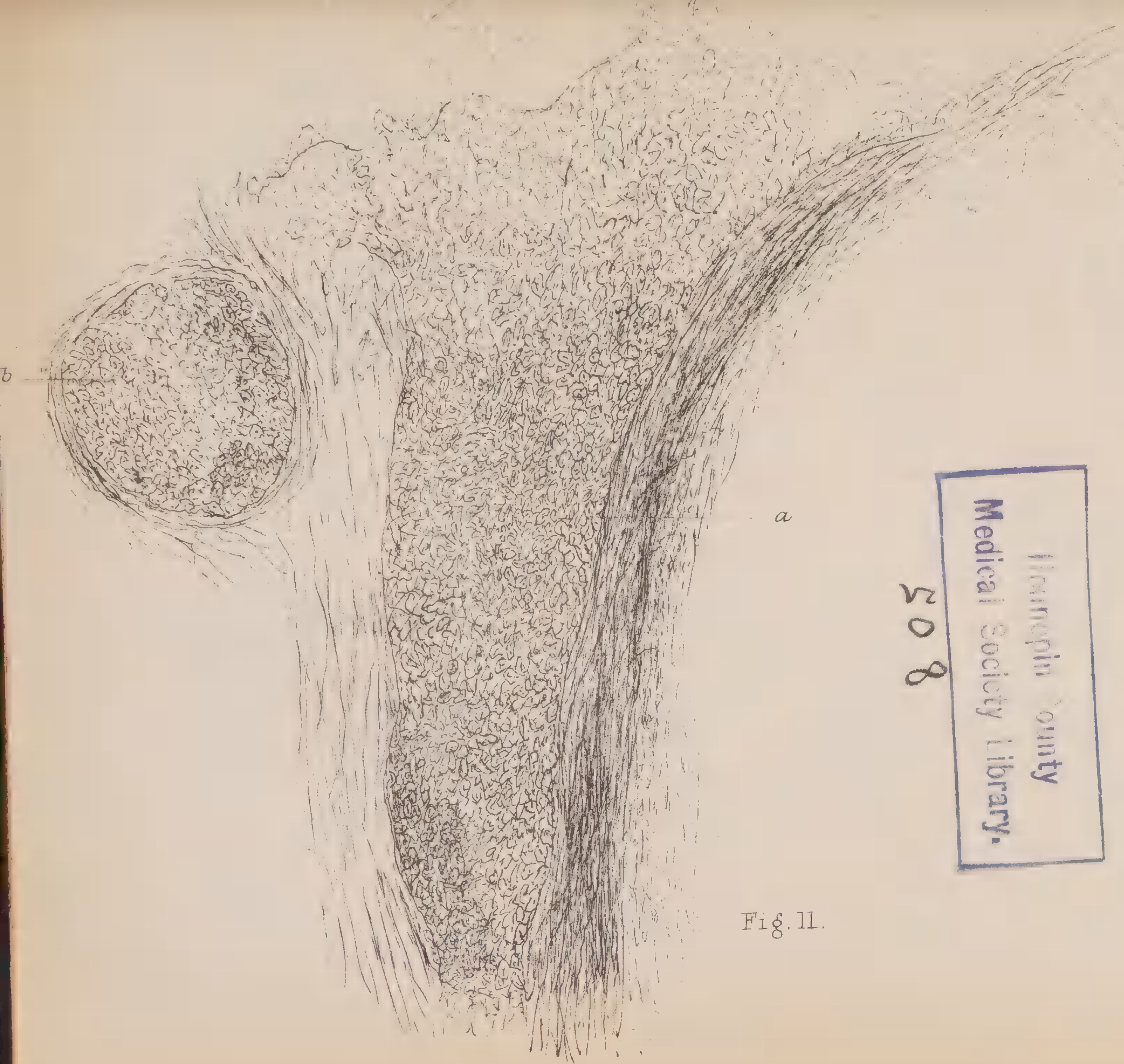

Fig. 11.

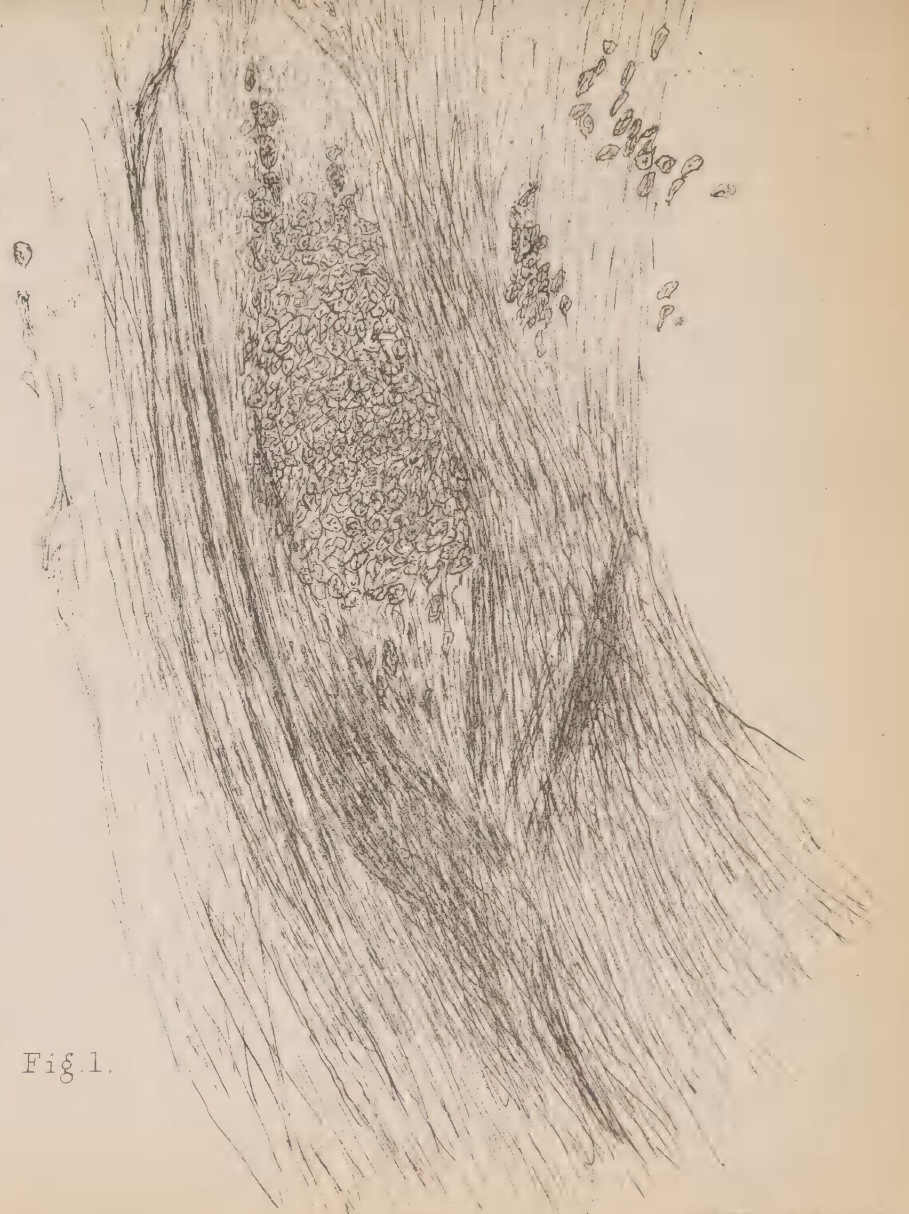

Fig. 1.

D^r J. D. Mann del^t.

W. West & Co. lith.

M^r Cullingworth's case of capsulated scirrhus.

Microscopical sections (250 diameters)

Jones, the pathological registrar at the Manchester Royal Infirmary. He reports that the tumour is surrounded entirely by a distinct thin fibrous capsule, and that its dimensions are as follow:

Circumference in the long axis	$9\frac{1}{4}$ inches.
,, in the short axis	8 ,,
Diameter in the long axis	4 ,,
,, in the short axis	3 ,,

It is nodulated externally, and internally it presents a greyish, firm, fibrous appearance, and is irregularly and somewhat indistinctly divided into lobules. Under the microscope it is shown to be composed of a fibrous matrix and of cells. The fibrous tissue is, in some places, collected into bundles, and in others forms a delicate stroma infiltrated with cells (fig. 1).

The cells are round and nucleated, and vary in size from $\frac{1}{2000}$ in. to $\frac{1}{3000}$ in. In some parts they form cylindrical masses bounded by a delicate membrane; these masses somewhat resemble gland-ducts, filled with cells (fig. 2, *a*). No trace, however, of lining membrane can be detected. Besides these elongated groups of cells, there are seen rounded masses, corresponding in size to ordinary mammary lobules, and distinctly enclosed by a membrane (fig. 2, *b*). The cells included in these have the same general character as those distributed through the stroma. In several of the masses spoken of as lobule-like the lining membrane is interrupted and the cells invade the surrounding stroma. In some portions the cells are less crowded, of a larger size, and show a well-rounded outline, and a distinct single or double nucleus, with granular contents. In cross-section of blood-vessels small cells make their appearance in the walls.

The report concludes as follows:

"The elongated and rounded masses of cells closely resemble gland-ducts, with their termination, and leave but little doubt that they are the remains of the gland in which the growth took place. Although the cells are smaller than those generally found in scirrhus, yet their distribution within a fibrous stroma places the tumour under the head of cancerous growths."

Dr. Julius Dreschfeld and Dr. J. D. Mann, of this city, have examined the preparations, and entirely confirm the accuracy of the above description. To the latter of these gentlemen I am indebted for the careful drawing which accompanies this paper.

Remarks.—I have already said that my first diagnosis, in the hurry of out-patient work, was that the tumour was non-malignant. I formed my opinion from its slow growth, its extreme mobility, the ease with which it could be separated from all the surrounding tissues, and the freedom from implication of the skin, the nipple, and the adjacent glands. When, however, I was enabled to make a more thorough examination in the wards, and found that there was

an irregular, nodulated, movable tumour in the epigastrium, and that the vomiting did not yield to rest and appropriate treatment, it appeared evident that there was cancer of the stomach, and then the exact nature of the mammary tumour became an exceedingly interesting and difficult question. Was it a simple mammary glandular tumour, coexisting with a tumour of the pylorus, as to the cancerous nature of which there could scarcely be any doubt? Was it, on the other hand, a slowly growing, encysted, scirrhous cancer? Or, again, was it originally an adenoma, which had subsequently undergone cancerous transformation? These were the questions that suggested themselves, and which the microscope alone could solve with certainty.

The rapidly fatal termination of the case enabled me very shortly to gain more precise information.

The post-mortem examination revealed, first of all, the existence of scirrhus of the pylorus, the diagnosis of which had been so positive as only to need this confirmation. Secondly, the tumour of the breast was found to be surrounded by a distinct thin fibrous capsule, and to present microscopic appearances, already figured and described, which proved it to be cancerous; so that we have here the very unusual phenomenon of a capsulated scirrhous tumour of the breast. In proof of its extreme rarity it will only be necessary to quote from two great authorities, the one M. Velpeau, and the other Sir James Paget.

"Scirrhus," says Velpeau, "is never isolated in the mamma; it is always continuous by some outgrowth, or root, with the mammary tissue;"[1] and again, "At first, scirrhus, even more than encephaloid, is always confounded with the tissues or the constituent parts of the affected organ. A genuine scirrhus *has never been seen* to roll or to glide amongst the organized structures which surround or are in the neighbourhood of it."[2]

Paget is no less clear. He says, "A scirrhous cancer in the breast has no distinct or separable capsule of cellular tissue investing it . . . .";[3] and afterwards, when speaking of medullary cancer in the breast, "The tumours are separable masses, closely connected with the surrounding mammary gland or fat, but not incorporated with them, and having, in some instances, distinct thin capsules, *a character at once distinguishing them from all the scirrhous cancers of the breast that I have yet seen.*"[4]

In the face of language so unmistakable it behoves one to be

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[1] Velpeau, 'Treatise on the Diseases of the Breast.' Translated from the French by Mitchell Henry (Syd. Soc.). London, 1856, p. 296.

[2] Loc. cit., p. 415.

[3] Paget (J.), 'Lectures on Surgical Pathology.' Third edition. Revised and edited by W. Turner. London, 1870, p. 612.

[4] Ibid., pp. 677-8.

very careful indeed before announcing the occurrence of an exception to the absolute rule thus laid down. I am very glad, therefore, to be able to present the reader with the foregoing engraving of two microscopic specimens, carefully mounted by Mr. Jones, which will, I think, leave no reasonable doubt in the mind of any skilled observer as to the tumour being scirrhous.[1]

There still remains the third question to be answered. Was this tumour originally a glandular tumour, or was it cancerous from the beginning? In endeavouring to decide I shall be guided on the one hand by the opinions of our greatest authorities on the general question of the possibility of such a transformation, and, on the other, by weighing the probabilities in this particular instance.

Velpeau, in his work already cited, speaking of those who "maintain that they have often seen innocent tumours become malignant," goes on to say, "M. Martin-Solon, seconded by Blandin, published in 1844 the account of a tumour in a woman aged 45, which had remained in a benign condition nearly twenty years, and then become transformed into encephaloid. Similar examples have been reported by others, especially by M. Roux. To this first case . . . two things may be objected—1st, there is nothing to show the innocent nature of the tumour in the first period of its history; 2nd, neither is there any absolute proof that it was really encephaloid at the time of its removal."[2] "It is my belief," he affirms in another place, "that these mammary glandular tumours remain innocent to the last, and are not susceptible of malignant or cancerous transformation."[3]

Paget says that he is not "aware of any facts which prove what is commonly believed, that after a time these" mammary glandular "tumours may become cancerous." In the next sentence however (and I wish to call special attention to this), he states his opinion that "such things may happen; and, on the whole, one might expect that if a woman have a tumour of this kind in her breast, cancer would be more apt to affect it as a morbid piece of gland, than to

[1] While preparing this account for publication a case was recalled to my recollection which excited a good deal of interest at the Leeds General Infirmary in 1863, during the time I was a student attending hospital practice there. A capsulated tumour of the breast was removed in the operating-theatre, and handed over for microscopic examination to the late Dr. R. G. Hardwick, physician to the infirmary, and Mr. Jessop, the then house-surgeon, both of whom were experienced microscopists. They pronounced it to be cancerous. In reply to a note I addressed to my friend Mr. Jessop (now surgeon to the infirmary), he says, "I remember in Dr. Hardwick's time a single case of cancerous breast tumour, distinctly capsulated, in which the structure partook in spots of the scirrhous character, and in other places of the encephaloid. The specimen was sent to Paget, whose assistant (I forget his name) confirmed our opinion." I very much regret that the want of fuller details prevented me from incorporating this—the only example I can meet with—into the text of my communication.

[2] Loc. cit., p. 301.

[3] Loc. cit., p. 322.

affect the healthy gland. But, I repeat, I know no facts to support this, and some that I have met with are against it."[1]

Erichsen, on the contrary, is of opinion "that cystic and adenoid tumours may remain for a long time in a benign and passive state, and then assume malignant characters,"[2] and he adduces the following case in support of this view.

In January, 1858, a woman, aged 48, was admitted into University College Hospital, under Mr. Erichsen, with a cystic tumour of the breast. She had first observed it when she was twenty-seven years of age, and at the end of fifteen or sixteen years it had slowly attained the size of a fœtal head. One of the cysts had now ulcerated, and there oozed from the opening a thin discharge. The general health was good. Except around the ulcer there were no adhesions to the skin, the tumour was freely movable, and there were no enlarged glands in the neighbourhood. An operation was proposed and declined. In six months, however, the patient "returned with a large ulcerated cavity in the centre of the tumour, and fungating masses sprouting from the bottom of it," and she now consented to have the gland extirpated. The tumour, after its removal, was found to consist of "several large cysts, of the size of pigeons' eggs, containing turbid but light-coloured serous fluid. The central portion of the tumour, and that at the base of the fungus, were solid, grey, and rapidly undergoing softening and disintegration. On squeezing this portion of the mass (the base of the fungus) a milky juice exuded; and Dr. Harley, who examined the tumour, stated it to be encephaloid. The sarcomatous structure beyond this, constituting the general mass of the tumour, appeared to be adenoid. The surface of the fungus, where exposed and protruding beyond the cyst, was epitheliomatous.

"Now, here were cystic sarcoma, encephaloid, and epithelioma, associated in one growth. . . . . The history of this case, the very lengthened period (more than twenty years) that the tumour had existed, the absence of all constitutional cachexy, of deep adhesions, or of glandular enlargement, or other secondary deposits, and its

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[1] Paget, loc. cit., p. 565. The passage continues—"Thus, in the museum of St. Bartholomew's is a portion of breast from a woman thirty-two years old, in which there lie, far apart, a small mammary glandular tumour that had existed four years, and a hard cancer that had existed four months. A second specimen shows a hard cancer and a proliferous cyst in the breast of a patient who died, some time after its removal, with recurrence of the cancer. A third case, just like the first, was under Mr. Stanley's care. In these cases, at least, the tumour was not selected as the seat for cancer, and I believe that they are not counterbalanced by any of an opposite kind." A case of a similar kind occurred to Mr. Simon, and was reported by him to the Pathological Society ('Trans. Path. Soc.,' vol. iv, p. 267). In the centre of the breast was a scirrhous cancer, two inches by one inch and a half, while the axillary margin of the gland was rendered irregular by a number of knots and pedunculated outgrowths, whose structure was entirely glandular.

[2] See a clinical lecture, 'Brit. Med. Journ.,' April 14, 1860, p. 281.

appearance only six months previous to removal, all pointed to primary simple cystic disease of the mamma, in which, as the result of secondary changes, encephaloid had developed itself, being an instance of the conversion of a simple into a cancerous tumour of the breast."

Mr. Skey, too, speaking after a vast clinical experience, is of the same opinion as Mr. Erichsen.[1] Now, what facts have we in favour of the belief that the tumour I have described commenced its career as a simple glandular tumour? Let us look, first, at its history, and then at its external characters.

History.—The patient had known of its existence for seven years, yet at the end of that time the external characters, as I shall show immediately, were not those which are ordinarily associated with scirrhus, and there was no enlargement of the neighbouring glands.

External characters.—The tumour was capsulated; it glided freely upon the subjacent parts and beneath the skin; the skin was not adherent or dimpled, and the nipple was not retracted.

These facts, taken together, show one of two things—either that this is a scirrhous tumour partaking much more largely of the characters of an innocent growth than any I have been able to find described; or that it was born an adenoma, and, having afterwards degenerated into a scirrhus (as a consequence, probably, of the development of scirrhous cancer in the stomach), still retained many traces of its original innocency. I think the latter will be allowed to be the more probable hypothesis.

To the first of the objections raised by M. Velpeau in the passage already quoted, viz. that there is no actual evidence of the original innocency of the tumour, I reply that such evidence is impossible from the very nature of the case. All we can do is, as I said before, to weigh the probabilities in any given instance, and, after all, the conclusion to be drawn will remain a matter of opinion, and not a demonstrated fact. I do, however, venture to think, that in the case I have here recorded, the presumptive evidence in favour of the theory of transformation is as strong as is ever likely to be forthcoming.

Cases have occasionally been reported[2] where cancer has followed the removal of mammary tumours of an innocent nature; an occurrence which Sir James Paget explains by supposing that the local

[1] See a clinical lecture by Mr. Skey, 'Med. Circular,' xv, p. 26, where the following passage occurs:—"I believe that benign diseases of the breast often degenerate into those that are malignant. . . . . Though I doubted it at one period, I have had several cases recently bearing on this point."

[2] A case of Mr. Erichsen's is reported in the "Mirror of Hospital Practice," in the 'Lancet' for Jan. 31st, 1852, where, two months after the removal of a cystic sarcoma from the right breast of a woman about thirty-five years of age, she presented herself with a nodule in the cicatrix, and at the time of reporting (six

injury, inflicted by the operation, renders the breast specially apt to become cancerous where a latent or hereditary proneness to cancer already exists in the patient.

"Such events . . . are enough," he says, "to suggest great caution in operating on the breasts of those who may be suspected to be by inheritance peculiarly liable to cancer." A practical deduction which every one will admit to be perfectly sound, if drawn from these cases alone. If, however, the evidence brought forward in Mr. Erichsen's case and in my own be accepted as reasonably conclusive, the possibility of cancerous transformation of innocent tumours is well-nigh established. The merely negative evidence of cases having been known where innocent and cancerous tumours were coexistent in the same breast (to which reference has been already made) cannot be of any weight as against evidence of a positive character. And this brings me to what I hold to be the practical lesson of the case before us.

If chronic mammary tumours may degenerate, as seems probable, in persons of a cancerous predisposition, into scirrhus or encephaloid, the risk of cancer appearing after operation is at least counterbalanced by the risk of cancerous transformation of the tumour if left to itself, so that the question as to the propriety of removal under such circumstances will require reconsideration.

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months after the operation) the breast had assumed the appearance of malignant ulceration. In a letter which appeared from Mr. Birkett in the following number of the 'Lancet' reference is made to a case which was observed in the clinique of Prof. Chelius, and reported by Dr. Bruch ('Die Diagnose der bösartigen Geschwülste,' case ix, s. 94), where a cyst containing fluid and intra-cystic growths was removed from the right breast, and after an interval of twelve years a cancerous tumour formed in the same situation, was removed, reappeared, and eventually caused the death of the patient. Mr. Erichsen mentions a case which occurred in his private practice ('Lancet,' Feb. 14, 1852, p. 184), where, three months after the removal of a chronic mammary tumour from the breast of an unmarried lady, aged 40, hard nodulated masses appeared in the cicatrix, and death occurred in five months after the reappearance of the disease. A fourth case was brought before the Pathological Society by Mr. Simon ('Trans. Path. Soc.,' vol. iv, p. 267; see also 'Medical Times,' vol. xxvii, 1853, p. 317), "where there had been removed from a woman's breast at the close of suckling a tumour which her medical attendant called a lacteal concretion; subsequently, along the axillary margin of the breast there occurred a disease (for which Mr. Simon removed the outer half of the gland) which . . . presented the characters of lobular hypertrophy; finally, she came a second time under Mr. Simon's care, about a year after this operation, and died with cancerous growths in the axilla, in the opposite breast, in the integument, and in the internal organs."

Chronicle of Medical Science.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.D., F.R.S.

I.—Toxicology.

On Poisoning by Chloral Hydrate.—In these reports attention has been called on previous occasions to the dangers arising from the improper use of chloral hydrate. We have now to recur to this subject and to report upon a case in which a patient was rendered insensible from a dose of the narcotic of not less than 240 grains and recovered. The subject of this accident was a middle-aged man of full habit and vigorous constitution. He had suffered from neuralgic pains, and had been also subjected to severe mental labour, from his professional work. From time to time he had taken chloral as an anodyne and could bear a dose of from forty to sixty grains with what he considered benefit rather than impunity. After a few days of unusual fatigue and worry he became quite sleepless at night-time, and after two restless nights he commenced early on the morning of the third day, during which the symptoms had continued, to take chloral. His first dose was fifty grains, and as sleep did not follow it so soon as was desired he took twenty grains speedily after, certainly within the hour. A bottle containing the remainder from half an ounce was in his room, and a little later still, while in a semi-conscious state, he mixed the whole of the contents of the bottle with water and swallowed the draught. For several hours after this he was supposed to be simply sleeping, the repose was so placid and so perfect. But at length the symptoms took a more serious turn; it was found that he could not possibly be roused, and the breathing became stertorous. Two hours after this, and nine hours after the last dose of the chloral had been taken, the patient came under the observation of the reporter. The symptoms were as follows:—The body was flaccid and the limbs were easily moved, remaining in the state in which they were left, as is seen in catalepsy. The temperature of the body in the mouth was 98° F., in the axilla 97°; over some parts of the surface of the body there was an inclination to perspire, and on the brow there was at times a free exudation of water, the water

standing out in large drops. The face was intensely flushed, the ears carmine red, and the redness of skin was, indeed, more or less perceptible over the whole of the body. When the skin was firmly pressed with the finger over parts where there was resistance of bone beneath, the blood, on removing the pressure, rushed into the part, giving a peculiarly well-defined, deep red mark, which lasted from three to four minutes. The pupils were dilated and refused to contract even under the influence of a strong light. The body over its entire surface was insensible to impressions, and the senses were equally deadened. The coma was deep, as in apoplexy, and the breathing was stertorous but not blowing. No efforts could arouse the sleeper. The odour of the breath was faintly ethereal, somewhat of the odour of methylic ether; the breathing, though stertorous, was regular, at twenty-eight per minute, and the auscultatory sounds were clear, except that in the regions of the larger bronchial tracts there was at intervals a faint mucous râle. The pulse was soft, but full and steady, giving sixty-four strokes per minute. The sounds of the heart were normal, but occasionally there was slight attempt at intermission in the stroke. Unfortunately a sphygmograph to take an arterial tracing was not at hand.

These were the leading symptoms in this case; they were sufficiently clear to supply the diagnosis of chloral narcotism before the facts about the administration of the agent were related. The treatment followed was that we originally suggested for such cases. The danger of death is from condensation of fluid in the minute bronchial canals from decrease of animal temperature, and the first point insisted upon, therefore, was to sustain, by every available means, the already failing animal heat. The bladder was watched, that it should not become distended with urine, and warm food (milk) was administered into the stomach while still the patient was unconscious. After seven hours, during which time the case was carefully observed, some signs of restless movement indicated a rallying from the lethargy, and soon afterwards the sleeper, though he knew nothing of the fact afterwards, rose, with assistance, and passed urine. Then he swallowed drink and again sank into deep slumber, but without stertor. Six hours later he again awoke more conscious; he passed urine and took fluid flood. He reclined once more to sleep, and on the following visit, next day, twenty-six hours after partaking of the narcotic potion, he was still drowsy and disinclined to move, but was conscious and capable of helping himself to move and to swallow. He had no remembrance whatever of the events that had occurred nor of the time that had elapsed. He made a rapid and perfect recovery.—*Reporter of Forensic Record of* '*Brit. and For. Med.-Chir. Review.*'

Analysis of the Air of Rooms covered with Arsenical Wall-Paper. —Several illustrious scientific men, as Taylor, Bunsen, and Pettenkofer, consider that the poisoning which results from arsenical pigments on wall-papers is due to fine particles removed by

mechanical causes from the paper, which particles mixed with the air of the apartment enter into the respiratory and digestive organs and thus induce poisonous symptoms. From this it has been inferred that danger can only arise from paper hangings in which arsenical colours are loosely attached, and that the air is not contaminated by such colours when they are varnished or applied as oil paints.

On the other hand, there are many observations of poisoning occurring in rooms where the arsenical paper has been covered with other paper completely innocuous, or where the walls are painted with an arsenical pigment ground in oil. It is, therefore, probable that a gaseous arsenical compound, arsenide of hydrogen or oxide of kakodyl, escapes from the colour, and the experiments performed by Professor H. Fleck, of Dresden, induced Dr. Hamberg, of Stockholm, from whose able report we are now extracting, to make further investigations on this important and interesting subject.

Dr. Hamberg's researches were performed in summer weather, in a residence which was let to him and his family in 1873. The room taken for the experiment was a large one, with two windows opening to the west; its walls were dry, and the papering of the room, according to report, had been put up twenty or thirty years. No smell or damp was perceptible; and the persons who let the rooms said they had not observed, either in themselves or others, any injury to health which could be ascribed to the green paper.

In testing the air of this room Dr. Hamberg drew the air from it by means of aspirators through cotton wool, till it was quite freed of dust, and then through a solution of silver nitrate in bulb tubes. The solution held one part of the silver salt in forty parts of water. During the experiment the doors and windows of the room were shut; the passage of air through the set of tubes was continued daily from the 16th of July to the 16th of August, when it was stopped. At this time 2,160,000 cubic centimètres of air had passed through the set of tubes.

When the air had passed through the silver salt solution in the bulb tubes for a week, a black sediment was observed in the solution. On examination by analysis a trace of arsenic was detected in the cotton wool in one of the tubes. The solutions of nitrate of silver through which the air had passed were also found to contain a small amount of arsenic.

The author infers from his experiments that arsenic escaped from this wall-paper in the form of arsenite of hydrogen. The paper on the walls was of a light green colour, with an ornamental pattern of brownish-yellow. The brownish-yellow colour was ferruginous, probably some ochre. The green colour resembled Schweinfurt green, and was proved to contain a considerable amount of arsenic. The colour of the paper had undergone a gradual alteration in composition; one part of arsenious acid had been oxidized to arsenic acid, while another part had been reduced and had combined with hydrogen. The author states, in conclusion, that none of the

family who resided in this house experienced any injurious effects from the inhalation of the air, but that he himself, who had a bedroom beside the room in which the experiments were made, and who often during the night had the door open, experienced in the morning a sense of heaviness in the head and of weariness. He also had an attack of rheumatism in the lower limbs during the month of July, but is unable to say whether these symptoms were caused by the inhalation of the air of the room from which the gaseous arsenical evaporation was going on.—*Pharmaceutical Journal*, August 1st, 1874.

[We notice these experiments as the first published in England (as far as we remember) in which the escape of arsenic, in the gaseous form, from wall-paper has been suggested. It is of moment that the statement be confirmed.]

Toxicological Action of the Alkaloid Veratrolla.—C. L. Mitchell has gained the St. Ebert prize for an essay on the officinal veratrums read before the twenty-second annual meeting of the American Pharmaceutical Association. Mitchell divided his essay into three parts:—In the first part he reviewed the characteristics of the veratrums; in the second part he gave a chemical history and examination of each variety of veratrum, with the reactions and products; in the third part he detailed the physiological and toxicological action upon rabbits and other animals of the chemical substances he had isolated. From his researches he draws the following conclusions:

1. There exists no such alkaloid as viridia.
2. Bullock's viridia is identical with and probably is jerira.
3. There is a distinct alkaloid in *Veratrum viride* differing from veratria and veratroida.
4. The resin of *Veratrum album* is in itself nearly inactive, and owes whatever power it may possess to the presence of veratrolla.

The alkaloid veratrolla, which exists in the *Veratrum viride*, is—(*a*) a local irritant; (*b*) an irritant, emetic, and cathartic; (*c*) a direct depressant of the circulation; (*d*) a powerful nerve and muscle poison, producing death either by suspension or paralysis of the heart muscle. It is probably a spinal and motor depressant like the other alkaloids of the same group.—Ibid., October 17th, 1874.

Fallacy of the Physiological Tests for Organic Poisons.—MM. Pietro Albertone and Filippo Lussana call in question the mode that has of late years prevailed of applying the physiological tests in cases of poisoning by organic substances, in which the poisons do not admit of being detected by chemical analysis. The arguments the authors adduce are that minute quantities of organic poisons may be produced in the secretions and tissues of animal dead bodies by the natural processes of decomposition and in a state of concentration, and that the poisons thus spontaneously originated may, by admission into the healthy body, cause symptoms resembling those that follow some of the better-known organic

poisons. In the case of two ladies who had been suspected of dying from the action of a poison, the aqueous extracts of their abdominal viscera and intestinal matters were tested side by side with the extractives derived from Liebig's extract of meat and from the bile of dogs. The substances thus used were injected into the veins and into the cellular tissue of living animals—viz. dogs, rabbits, and frogs.

The conclusions at which the authors arrive are thus summarised:

1. The physiological process does not, as a criterion, authorise the determination of an extraneous poisonous principle in examined substances.

2. But it also does not authorise the exclusion of the possibility of the pre-existence of extraneous poisonous principles alterable or capable of dispersion by time or by fermentative or analytical processes.

3. Neither does the process exclude the possibility of the existence of a poison having an action analogous to that of the extractive principles of the viscera in a state of concentration.

4. Neither, again, does it exclude the possibility of the presence, in extremely small and subdivided quantities, of a poisonous principle.—*Gazetta Medica Italiana*, tome 52, and *Annali di Chimica*, Agosto, 1874.

II.—Forensic Medicine.

On a Method, easy, simple, and certain, of distinguishing Real from Apparent Death.—Dr. Ange Monteverdi, of Cremona, relates a series of researches made upon men in the death struggle and upon the dead body with a view of discovering a method at once simple and sure for determining actual from simulated death. He was led to undertake this study from an observation on a choleraic patient to whom he had administered an injection of ammonia by the hypodermic needle in 1867. The phenomenon consisted of a spot or blotch which appeared in the skin at the point where the ammoniacal injection had been made. This the author maintains is always present while there is any remaining vital action, but it ceases with death. The cessation of function in the body, altogether, is not, however, universal at the same moment. The capillary vessels may retain their activity some time after the motion of the heart has ceased, a fact which indicates that the sanguino-capillary circulation represents the last perceptible act in the life of man, and that the nutrition of the tissues is due to the plasma transuded through the walls of those vessels. The capillary circulation, therefore, ought, in physiological death, to cease its functions after the cessation of the organic function. Thus, there is an intimate connection between absorption and the capillary circulation; the one cannot exist without the other, and when by the use of ammonia the cessation of absorption is indicated death is absolutely indicated.

The process pursued by Dr. Monteverdi in carrying out his

experiments is to use an ammonia solution of a specific gravity 0920. He fills the hypodermic syringe with the solution and injects all the liquid under the skin, and if very determinate action be required he repeats the process in the same place and watches for the local results, from which he judges whether or not absorption is or is not being carried on—in other words, whether or not death has actually occurred. He gives the narration of several cases in which the injection was conducted in the living body during the act of death and at various intervals after death, but does not supply what is most wanted, viz. cases such as those of catalepsy, in which death is actually simulated so closely as to seem to be real.

The following are the conclusions at which he arrives; they include the salient facts shown in the explanation of his experiments with ammonia:

(*a*) The last act of the organic life of man consists in absorption, a function which only ceases with the cessation of the circulation of the blood in the capillaries.

(*b*) Liquid ammonia is the only agent known at the present time suitable for certifying the activity or cessation of absorption and consequently of the capillary circulation.

(*c*) When the part upon which the ammoniacal injection is made is in full vitality, the vitality shows itself immediately by an effect of vital reaction, by a blotch of a red erysipelatous colour, the dimensions of which far exceed the ordinary dimensions of blotches observed during the death struggle and after death.

(*d*) During the death struggle the injection of an equal quantity of ammonia gives rise to a red wine-coloured spot, of an oval, elliptical, or almost circular shape, of from five to seven centimètres in diameter, with a slight serous exudation which raises the epidermis at certain points; and with bulbous specks sensible to the touch and of a spotted form.

(*e*) During the intermediate state of death (which, according to Thiery, immediately follows the death struggle, and in which still exists a stage of life entirely imperceptible, reduced to the most feeble degree, and which in its turn is extinguished little by little) the ammoniacal injection induces a spot similar to that obtained during the dying struggle, but of a wine-red colour less intense and generally of more restricted proportions.

(*f*) At actual death—that is to say, when death is materially and rigorously certain—the ammoniacal injection produces a uniform blotch without specks, without trace of any wine-red colour, of a dirty skin colour, and of dimensions almost equal to those of the blotches produced during the death struggle.

(*g*) During the death struggle, as well as during the intermediate state of death, the blotch commences to produce itself immediately after the injection of the ammonia, and ordinarily completes itself in the space of from a quarter to half an hour. On the other hand, during actual death the blotch commences at the soonest after the lapse of ten minutes, and is not complete for from half an hour to an hour afterwards. This slowness becomes greater

and greater in proportion as the distance of time which separates the moment of injection from that of death is more considerable.

(*h*) The blotch spoken of under section *f* constantly shows itself on dead bodies, and furnishes the only criterion of actual death which is known at the present time. When five hours have elapsed after the time of the presumed death, it can be concluded with certainty that there is actual death when a blotch of a dirty skin colour without any trace of wine redness is seen to follow the subcutaneous injection of ammonia.—*Paper read before the Medical Society of Cremona*, and separately published, July, 1874.

Diagnosis of Blood-stains.—Dr. Joseph C. Richardson favours us with another of his able and laborious forensic essays on the diagnosis of blood-stains. He maintains in this paper that the red blood-discs of animals, with rounded corpuscles, are just as distinct in different animals as are different kinds of shot. For instance, the red globules of man's blood are nearly twice the size of the sheep, and about four times that of the musk deer, just as No. 1 shot is double the magnitude of No. 5 and quadruple that of No. 8.

Respecting change of size resulting from drying of the corpuscles the author remarks that the assertion of Virchow, that a man's life should not be put in question on the uncertain calculation of a corpuscle's ratio of contraction by drying, does not seem to be a fair statement of the point at issue, because, since the red blood-corpuscles of oxen, horses, pigs, sheep, deer, and goats, are all much smaller than those of man, no degree of *contraction* which they could undergo would render the stains in which they occur *more* liable to be mistaken for man's blood; and if, as is rarely, if ever, the case, human red blood-corpuscles in a stain were by any means contracted so as to resemble those of an ox, for instance, in size, the evidence from microscopic examination would only mislead us into assisting in the acquittal of a criminal, and could not betray us into aiding to convict an innocent person.

The author's observations, made upon many different kinds of blood, and under a great variety of conditions, clearly indicate that the cell-wall of a red blood-globule is nearly or quite inelastic, and incapable of any marked expansion by the process of drying or moistening with the fluids he recommends for the examination of blood-stains. The slight increase of size mentioned above as occurring in the desiccation of a thin film of blood forms only an apparent exception, and is probably due to a change of shape taking place during the complete flattening out of the discs as they lose their contained water.

But all these theoretical considerations are of very secondary importance in comparison with the positive fact as to whether practically we can or cannot discriminate the stains of human blood from those made by the blood of oxen and sheep. The author has, therefore, endeavoured to *work out* a conclusive answer to this question, obtaining it by a method which will, he trusts, carry conviction to the mind of every honest seeker after truth.

On the 16th of May, 1874, Prof. J. J. Reese and Dr. S. Weir Mitchell each prepared for him three packages of dried blood from stains made by sprinkling the fresh fluid from an ox, a man, and a sheep, upon white paper. The two series were simply numbered 1, 2, and 3, and a memorandum preserved by each gentleman, specifying which kind of blood composed each sample. By this plan it is obvious that he was prevented from having any clue to the origin of the specimens save that afforded by the microscope, and his examinations and measurements were, therefore, entirely free from bias.

Some small particles from specimen No. 1, handed him by Prof. Reese, were broken up into a fine dust with a sharp knife, upon a slide, and covered with a film of thin glass. A few drops of the ordinary three quarters of one per cent. common salt solution were then successively introduced at one margin of the cover, and removed from the opposite edge, as they penetrated thither, by a little slip of blotting-paper, thus washing away the colouring matter from the tiny masses of dried clot. When these particles were nearly decolorised a drop of aniline solution was allowed to flow in beneath the cover, and, after remaining about half a minute, was in its turn washed away, and its place supplied by a further portion of weak salt solution.

On adjusting the specimen as thus prepared under a $\frac{1}{25}$ immersion lens (giving an amplification with the A eye-piece of 1250 diameters), a fragment of the blood-stain was soon discovered, which displayed the delicate cell-walls of its component red and white corpuscles. Ten consecutive red discs from these, selected simply as among those which had become but little distorted, were found to measure as noted below in the first column. The second and third rows of figures show the result of similar experiments, performed on samples 2 and 3, all the magnitudes being given in parts of an English inch.

Specimen No. 1.	Specimen No. 2.	Specimen No. 3.
1-3448	1-4762	1-5555
1-3572 (minimum)	1-4762	1-6060
1-3572	1-4878 (minimum)	1-5405 (maximum)
1-3572	1-4651	1-5880
1-3333	1-4878	1-6666 (minimum)
1-3125 (maximum)	1-4444 (maximum)	1-6060
1-3448	1-4444	1-5777
1-3278	1-4762	1-5555
1-3333	1-4651	1-5888
1-3448	1-4762	1-5777
1-3407 (mean)	1-4694 (mean)	1-5828 (mean)

Since the red corpuscles of human, ox, and sheep's blood measure, according to Gulliver, $\frac{1}{3200}$, $\frac{1}{4267}$, and $\frac{1}{5300}$ of an inch respectively, and previous experiments of his own had demonstrated a disposition to *slight* contraction in the corpuscles of blood-stains which have been dried and moistened again, he of course concluded that sample No. 1 was human blood, No. 2 was ox blood, and No. 3 was sheep's

blood. On reporting these diagnoses to Prof. Reese he had the satisfaction of learning that they were "entirely correct."

The highly refractive properties of glycerine and its solutions render it less applicable as a liquid for moistening blood-stains and bringing into view the delicate cell-walls of their constituent corpuscles than the 75 per cent. salt solution. But for preservation and prolonged study of specimens of blood-stains glycerine forms the best medium at our disposal.

In examining spots of blood more than one tenth of an inch in diameter fragments should be scraped from the edges or thinnest parts of the stain, because specimens from the central portions sometimes exhibit numerous fibrine filaments, which have appeared before the desiccation of the drop. These, of course, interfere with the investigation, by forming a more or less complete meshwork around the cell-walls, and so confusing the delicate outlines which the latter present when the view is uninterrupted.

As a contribution towards answering the question of how long after their deposit upon objects blood-stains may be detected by microscopic investigation, Dr. Richardson mentions that a fragment from one of the twenty blood spots used in May, 1869, "for estimating the delicacy of the microscopic test for blood," was recently examined as above described, and found at the end of *five years* still to exhibit multitudes of corpuscles, which could be clearly distinguished from those of the ox or sheep, as will be seen by the following record of measurements made May 23rd, 1874:

1-3572	of an	inch.	
1-3448	„	„	
1-3278	„	„	
1-3125	„	„	maximum.
1-3390	„	„	
1-3509	„	„	
1-3448	„	„	
1-3509	„	„	
1-3572	„	„	minimum.
1-3448	„	„	
1-3425	„	„	mean of ten corpuscles.

The corresponding average of measurements five years ago was $\frac{1}{3474}$ of an inch, so that no further contraction seems to result from age, and as the outlines of the corpuscles appear quite as distinct now as they did soon after the blood was drawn, it seems probable that this microscopic evidence of human bloodshed will be equally unmistakable twenty or even fifty years hence, provided due care continues to be exercised in its preservation from moisture and external violence.

In conclusion, the author submits that the results of his experiments above narrated *prove* that, since the red blood-globules of the pig ($\frac{1}{4230}$), the ox ($\frac{1}{4267}$), the red deer ($\frac{1}{4324}$), the cat ($\frac{1}{4404}$), the horse ($\frac{1}{4600}$), the sheep ($\frac{1}{5300}$), and the goat ($\frac{1}{6366}$ of an inch), are all so much smaller than even the ordinary minimum size of the human red disc, as measured in his investigations, *we are now able,*

by the aid of high powers of the microscope, and under favorable circumstances, to positively distinguish stains produced by human blood from those caused by the blood of any of the animals enumerated, and this even after the lapse of five years from the date of their primary production.—*Reprint from the 'American Journal of the Medical Sciences' for* July, 1874.

III.—Hygiene.

The Caisson Disease.—Under this title Dr. Andrew H. Smith defines the disease which is developed in those who work in the caisson or coffer dam, under increased atmospheric pressure. It is, he says, a disease depending upon increased atmospheric pressure, but always developed after the pressure is removed. It is characterised by extreme pain in one or more of the extremities, and sometimes in the trunk, and this may or may not be associated with epigastric pain and vomiting. In some cases the pain is accompanied by paralysis, more or less complete, which may be general or local, but is most frequently confined to the lower half of the body. Cerebral symptoms, such as headache and vertigo, are sometimes present. The above symptoms are connected, at least in the fatal cases, with congestion of the brain and spinal cord, often resulting in serous or sanguineous effusion, and with congestion of most of the abdominal viscera.

The morbid anatomy of caisson disease is thus defined. The constant lesion in fatal cases of caisson disease is congestion of the brain and spinal cord. The congestion may be evenly distributed, or it may vary in intensity in different localities. This is especially true as regards the cord. The congestion affects both the meninges and the substance of the brain or cord. In some instances extravasation of blood takes place. In most of the published cases there was found also more or less serous effusion in the arachnoid.

The tissues of the scalp and those surrounding the spinal column are sometimes engorged. Dr. Jaminet describes a case in which the tissues over the spine were congested, the vascularity increasing regularly as the dissection proceeded deeper towards the vertebral column.

When sufficient time elapses before death there may be softening of the brain, occurring in spots. This is probably due to occlusion of the vessels by coagula formed during the primary congestion.

Congestions also occur in other localities, and especially in the solid abdominal viscera. The liver and spleen have been found engorged in nearly every case. The kidneys, too, are usually congested, and in several cases described by Dr. Jaminet clots of blood were found extravasated in the organ.

The mucous membrane of the stomach, intestines, and bladder, is often found injected and marked with ecchymotic patches.

The lungs, in cases of true caisson disease, seldom present any other change than simple hypostatic congestion.

The one essential cause of the disease, without which it can never be developed, is *the transition to the normal atmospheric pressure*

after a prolonged sojourn in a highly condensed atmosphere. Hence we have to consider two elements, *pressure* and *time.* As the momentum of a moving body is found by multiplying the weight by the velocity, so the danger in these cases is as the degree of pressure to which the person has been exposed multiplied by the duration of the exposure. But inasmuch as a prolonged sojourn in the caisson does not in every case produce the disease (many of the men employed escaping it entirely), it follows that there must be concurrent causes which determine its development. This is what we observe in many other diseases of a specific origin. Thus, the essential cause of intermittent fever is exposure to a peculiar malaria, yet only a portion of those so exposed are affected by the disease.

The first of the concurrent causes of caisson disease is a *special predisposition.* This is occasionally strongly marked, some persons being affected by a short exposure to a low pressure from which there would generally be experienced no inconvenience whatever.

The study of these cases has led the author to the suspicion that they afford a key to the singular, though very common, predisposition to pains in the limbs on the approach of a storm. These pains are generally considered to be of a rheumatic character, and to depend upon the *dampness* of the atmosphere. But inasmuch as the disease we are considering affords examples of pains precisely the same in character, but immensely intensified in degree, resulting from the diminution of an atmospheric pressure to which the system had adapted itself, and irrespective of any question of humidity, analogy suggests that the so-called rheumatic cases are simply exaggerations of a predisposition, identical in kind with the one under discussion, and are produced by the low barometric condition of the atmosphere which precedes a storm, and not by the influence of moisture. It is true that the change in the pressure is insignificant when compared with that which produces the caisson disease, but it is supplemented by the immensely greater duration of the higher pressure to which the subject has been previously exposed. The author adds, as suggestions, the following:—He states that it is exceedingly desirable the men should be under control to a certain extent during the intervals of work. Excessive use of intoxicating liquors should be prevented, regular hours for sleep and meals insisted upon, and sufficient nutritious, digestible, and properly cooked food should be provided. The men should sleep in comfortable beds and in properly ventilated apartments. All this is manifestly unattainable if the men live in homes of their own choosing. If, therefore, any great work by the aid of compressed air is to be undertaken, the preparations for it should include whatever is necessary for housing and feeding the men at a convenient place near to the work. For this purpose temporary barracks may be erected in an enclosure, which the men should not be permitted to leave except under proper restrictions. The food should be furnished by the employers, be of good quality, embrace a sufficient

variety, and be prepared by competent cooks. Sleeping apartments should be provided, allowing at least 800 cubic feet of air space to each man, and with facilities for efficient ventilation.

A hospital should be arranged with a sufficient number of beds, and fitted with every appliance necessary for treating patients *during their entire illness.* The hospital to be in charge of a competent steward, under the supervision of a physician, who should attend a portion of each day.

Of course this implies that only single men shall be employed, and that they shall agree at the outset to submit to a *quasi* military rule.

Where the number of men is considerable, as would be the case in any large work, the company should carry out the above suggestions economically to themselves, and after deducting the cost from the pay of the workmen there would remain to the latter more than they would have left after paying their board in the usual way. At the same time the men would be so much more comfortable than if left to provide for themselves that they would value the position, and the fear of being discharged would be a sufficient restraint upon them. The tendency, too, would be toward securing at the beginning of the work a set of men who would continue to the end. These men, commencing when the pressure was slight, would not be affected by its gradual increase, thus avoiding the great danger which attends those who begin work for the first time after the pressure has attained a high figure. It is by attention to this point more than by anything else that the suffering and danger resulting from the use of compressed air may be diminished.

New hands should not be allowed to work in the caisson more than one watch in each day for the first week, after which half the usual second watch might be added for another week, at the close of which the full time could be entered upon.

Since much of the work on the pier above, as well as in the caisson below, is unskilled labour, it could readily be so arranged that the gangs could work on alternate days in the compressed and in the external air. The advantage of an interval in the caisson work is immediately apparent, the cases of sickness being notably diminished by even a single day's intermission.

The men should be instructed never to enter the caisson with an empty stomach; to use as far as possible a meat diet, and take warm coffee freely; always to put on extra clothing on coming out, and avoid exposure to cold; to exercise as little as may be during the first hour after coming out, and lie down if possible; to use intoxicating liquors sparingly, better not at all; to secure at least eight hours' sleep every night; to take care that the bowels are open every day; never to enter the caisson if at all sick; and to report at once at the office all cases of sickness, even if they occur after going home.

The men should be selected, as far as possible, from those who are of a spare and wiry build, and no one should be accepted who has a tendency to corpulency.

The lock-tenders should be selected with special reference to their trustworthiness, and if it is found that they cannot be depended upon the air-cocks should be so arranged that the aperture can be graduated from time to time by the engineer in charge, so as to prevent the possibility of a too rapid change of pressure.

In entering the caisson at least three minutes should be allowed in the lock for each additional atmosphere of pressure, and at least five minutes in coming out.

As for the number of hours of work daily, taking it for granted that twelve hours is the extreme time that a man can labour without detriment to health in any ordinary atmosphere, then with a pressure of two atmospheres, or 15 lbs. additional pressure, he can labour about one half that time; with three atmospheres, about one third of that time; and with four atmospheres, about one fourth of that time. In other words, the time is inversely as the pressure.

Whenever, for any cause, a sudden increase of pressure is demanded, the watch should be shortened to a corresponding extent.

The air-locks should be placed at the top rather than at the bottom of the shaft, in order that the stair may be climbed *in the compressed air*, instead of immediately after leaving the lock, when the system is more or less prostrated by the change taking place in the circulation. If, for any reason, it is impracticable to have the air-lock at the top of the shaft, an elevator should be employed to lift the men to the surface.

Care should be taken to maintain the air in the caisson at a sufficient degree of purity, as there may be a wide difference between the amount of air required to supply the necessary mechanical conditions for carrying on the work and the quantity demanded for the health of the workmen.—*Prize Essay of the Alumni Association of the College of Physicians and Surgeons, New York*, 1873, '*On the Effects of High Atmosphere, including the Caisson Disease.*'

On Ozone.—Professor Silvestro Zinno gives the following *résumé* of the properties and qualities of ozone. He says it is aeriform, incompressible, invisible, strongly negatively electrical, of a polarity communicable to other bodies (copper, silver), and smell and taste similar to a sea-crab. Compared with an equal volume of air its density is = 1658, compared with hydrogen it is = 48.

Light unaccompanied by a high temperature makes no change in it. Great heat reduces its bulk when carried beyond 150°. Repeated electrical discharges (perhaps because they heat it) reduce it to oxygen.

In water it is much less soluble than oxygen (O vol. 1 : aq. vol. 154). When made to bubble in boiling water it ceases to be ozone.

By its oxidizing power, so far superior to oxygen, it directly oxidizes almost all the chemical elements with the exception of

aluminium, gold, and some other metals. The less oxygenated acids perfect their oxidation with electrical oxygen; the inferior metallic oxides become superior oxides; some few, according to Schönbein, change it into ordinary oxygen. Salts not saturated with oxygen become saturated themselves with the ozone. Many non-oxygenated binary composite bodies oxidize more or less. Some sulphides become sulphates. Various chlorides, bromides, and iodides, may even become chlorates, bromates, &c. All the organic bodies undergo more or less an active oxidation through ozone. This is why it is a decoloriser and disinfectant.

Some essences, such as terebinth, cinnamon, and others, absorb it, especially in the light, and combine with it after a time.

The manner of determining the presence of azone is founded directly on its oxidizing properties, and in some cases its colouring properties.

Although many attempts have been made to discover an exact and safe means of determining the presence of ozone qualitatively and quantitatively, still Schönbein's method of the solutions of iodide of starch and of iodized-starch cards is the one still to be preferred.

The ozonoscopic cards may lead to errors by acid emanations as well as by aromatic effluvia and ammoniacal vapours., &c. In order to arrive at a greater degree of certainty Houzeau advised the doubly coloured cards, that is to say, with the tournasole and with the iodized starch solution. Others have advised the triple iodized starch cards, with the azure tournasole and the red. This method is reputed to be less equivocal.

Ozonoscopic observations in the present state of science, although they afford much interest, do not offer the value which they deserve. 1. Because an exclusive and exact method has not yet been selected for declaring the presence of ozone. 2. Because the observations can only be carried out on the summits of the meteorological observatories, whilst they ought to be going on at various places and at various heights in the same region, and, if possible, at every hour, by a mechanical system capable of issuing at every hour the ozonoscopic card which has been agitated by the air by the aid of the said mechanism. 3. Because, finally, the before-mentioned cards should be the same in every observatory.

From the observations hitherto made Professor Zinno infers that ozone is found more in elevated than in low-lying atmospheric regions, far from populated districts, at a distance from organic and organized emanations, and more frequently in remote maritime regions.

That it is to be found more in mild seasons and with certainty in the spring, and that the month of May shows the highest decided ozonometrical (quantity), and the month of November the lowest; and that as regards the coloration of the ozonoscopic cards, the progressive order of atmospheric ozone, always excepting unusual accidents, may be thus laid down:—November, February, October, December, January, September, July, August, June, April, March, and May.

Ozone acts upon animal substances, albuminoids, gelatinous sub-

stances, milk, serum, whey, chyle, lymph, water, bile, &c. It impedes the coagulation of the blood and changes blood from red brown to deep vermilion, but a continuous action of the ozone in perfectly liquid blood discolours it until it becomes a pale rose colour. Moreover, it entirely changes the blood-globules, ceasing in their presence to be ozone. The blood remains liquid by virtue of the ozone, because the albumen and the globuline, from which the fibrine is formed, are oxidized. Ozone, when breathed, attacks the respiratory organs more or less seriously, producing hyperæmia, pulmonary bronchitis, more or less violent and mortal. It does not, however, appear that ozone in doses of two milligrammes can cause the death of even a rabbit, as experimentalists have imagined.

It is generally sought to be able to establish that the excess of atmospheric ozone may be the cause of diseases of the respiratory organs, whilst deficiency of the same would determine diseases of the abdominal viscera.

During cholera epidemics atmospheric ozone has been either non-existent or constantly at a minimum. Ozone destroys miasma, hence the necessity of causing it to appear in endemial marshy places, by creating vast and special plantations. Ozone is recommended as a therapeutic and hygienic remedy against cholera and fevers.

It is desirable as a disinfectant and a discolorant. Pasteur proposes it to age wines, others as an antiseptic to prevent putrefaction; hence various methods have been suggested by Schönbein, Figuier, Bockel, Hoffman, Harris, Faye, Polli, Mantegazza, Selmi, Borselli, Guerri, Fasoli, and very many others.

If ozone could be obtained in large quantities and by economical means, a problem which seems already near a solution, chemistry, medicine, agriculture, commerce, and the arts, would avail themselves of it to a large extent.—*Il Piria*, 15th July, 1874, and *Annali di Chemica*, N. 4, Fasciolo di Ottobre, 1874.

IV.—Summary.

What Effect does Syphilis have upon the Duration of Life? By Frederick R. Sturgis, M.D. ('Separate Paper,' New York, 1874.)—This paper is in the form of a letter addressed to an editor who asks for information as to the effect of syphilis in reducing the value of life, with especial reference to life assurance. The author considers it unsafe to insure any patient in whom the disease is still present, however slight the symptoms may be. This, however, does not argue that the same man may not, after he has recovered from his disease, be a good risk. In fact, he believes that the disease is curable, and he assigns two years as the period required, on the average, to effect the cure.

Poisonous Action of the Flowers of Colchicum autumnale at the time of Flowering. By M. J. Pierre. ('Comptes Rendus,' lxxiv, p. 633.)—In this singular paper M. Pierre relates his observations on some flowers which he had plucked in the recent or flowering

condition, from the colchicum plant. He noticed that soon after he had removed his hands from the flowers his fingers changed colour, and became of that greenish or livid hue which is seen in the hands of those who are dead and are on the point of decomposition. The change of colour extended beyond the fingers, and lasted for a period of ten minutes. The experiment was repeated with the same results, but failed when the leaves had faded. Pierre thinks that the phenomenon is due to an unknown volatile substance which exhales from the leaves and is absorbed by the fingers.

A Case of Hydrophobia. By ALEXANDER HADDEN, M.D. ('Physiological and Medico-Legal Journal,' September, 1874.)—This case is one which in medico-legal circles in New York has created great attention and interest. A man named William McCormack, aged twenty-six years, died of hydrophobia, produced, as it would seem, from the bite of a little black dog, which showed no symptoms of disease, and which, in fact, after the death of the man was produced at a meeting of the Neurological Society. Dr. Hammond examined the brain and spinal cord of the man who had died and found fatty granular degeneration (with congestions and extravasations) in the cortical structure of the brain, the medulla oblongata, the spinal cord, and the roots of the pneumogastric, hypoglossal, and special accessory nerves. The case supports the view long since offered by Dr. Wright, of Birmingham, that the bite of a dog that shows no signs of disease may give rise to hydrophobia.

On the Mineral Oils as Disinfectants. By JOHN DAY, M.D. (Reprint from the 'Australian Medical Journal,' June, 1874.)—In this paper Dr. Day maintains that certain of the mineral oils are of great use as disinfectants, and especially the oil known under the name of gasoline. The value of these oils, he believes, depends on the fact that they are rich in peroxide of hydrogen. The gasoline is employed by Dr. Day in various ways as a disinfectant. He applies it to walls, to articles of furniture, and to clothing. The hands of the practitioner are also, he thinks, washed with advantage in this hydrocarbon, from which they should be allowed to dry in the open air. A peculiar and valuable property of the mineral oils as disinfectants is that of their being continuously in action. They improve and gather force by exposure to the air.

REPORT ON SURGERY.

By H. A. Reeves,
Assistant-Surgeon to the London Hospital, and Surgeon to the East London Hospital for Children, &c.

Varix of the Dorsal Vein of the Penis.—Dr. Jullien extracts some interesting cases from 'Les Dernières Publications du Dr. F. Parona,' Surgeon to the Novare Hospital; among them are the following:

Case 1.—A young man of lymphatic temperament, who had for a long time practised masturbation, noticed when attempting connection for the first time that he could not succeed, erection being absent. Chagrined, he returned to his former practices. After three years he again attempted coitus, and this time successfully, though incompletely. He contracted a blennorrhagia, and consulted a physician, who found that the seminal secretion was normal, venereal desire not deficient, and that the organs seemed normal; however, erection was very incomplete and sensibility of the glans penis appeared absent. He took it to be a nervous affection and commenced hydropathic treatment, which the patient religiously followed, but without effect. M. Parona was then consulted and made a careful examination, and discovered a varix of the dorsal vein, which was double its normal size, and to which he attributed the defective erection. Some days afterwards fifty centigrammes of a mixture of equal parts of water and chloral hydrate were injected into the vein, and two days after the operation the patient told M. Parona that he had had a complete erection during his sleep and on the fifth day after operation had successful coitus.

M. Parona remarks that the flaccid dilated condition of the vein prevented complete erection, and that turgescence of the glans and spongy and cavernous bodies could not be produced, as the sensibility of the glans was nil, and consequently that the erection which is caused by *reflexion* from this sensitive part could not be brought about.

M. Porta first proposed intra-venous injections of chloral hydrate for the cure of varices, and was successful in its practice. M. Parona has employed them several times and praises them without reserve. The advantages are simplicity, celerity, and durability of the cure.

Case 2.—*Cavernous angioma of glans penis.*—In April, 1871, a child, æt. 8, was brought to Dr. Parona; its general health was good, but it suffered from a tumour of the glans penis. Its parents had noticed the size of the organ shortly after its birth, and later on clots of blood were expelled with the first drops of urine, and the neighbouring part of the frænum commenced to form a prominence. The parents consulted several surgeons. One declared the tumour to be of a vascular nature and proposed amputation of the glans, but the parents refused; another took it to be a calculus impacted in

the glans and cut down on it, but only gave exit to a quantity of blood. The tumour increased in size, and when Dr. Parona was consulted the tumour, which had invaded the circumference of the meatus, presented well-marked characters of a vascular tumour, and as interference was imperative the parents consented. M. Parona partially excised the gland and applied pads dipped in perchloride of iron. A few days after the operation the organ became erysipelatous, and this lasted about one month; then the cicatrix became covered with little red prominences the size of a pin's head, which in spite of cauterization increased very rapidly; in fact, so great was the regrowth that Professor Bruns, of Turin, advised penotomy. M. Parona performed the operation in a manner which was a compromise between the methods of Rizzoli and Chassaignac, and very like that which M. Bourquet, of Aix, communicated to the Society of Surgery in 1867, viz. section of the urethra by a cutting instrument and of the cavernous bodies by the écraseur, bringing together of the skin and mucous membrane by three points of suture, and tying in a catheter afterwards. Recovery was very rapid. On examining the tumour the cavernous bodies were found to be strongly developed, but not altered; the parenchyma of the gland appeared in several points rarified and as if too spongy; the urethral mucous membrane was in its whole extent covered with granulations, the largest being the size of a millet seed. Microscopic examination showed spaces enclosed in a fibrous stroma, which contained cellular elements, a great quantity of dilated capillaries with thick walls rich in nucleoli and for the most part gorged with blood. In the points where the alteration was most marked the fibrous tissue was very scanty or completely wanting, the vessels ran together and became blended and confused and formed irregular spaces full of blood; the layers of the mucous membrane were thickened in certain points, in others invaded by vascular lacunæ which bulged to the exterior.

MM. Bizzozero and Gelgi declared the tumour to be an angioma. Was it simple or cavernous? M. Perona thinks it difficult to establish a distinction, as cavernous angioma is the result of the successive dilatation of newly formed vessels and their transformation into cavernous spaces. After extensive bibliographical research he has not been able to discover any description of an exactly similar case; the only cases which approach it are Erichsen's case, in which amputation of the penis was deemed necessary, and Rizet's. Among 151 cases of vascular tumour reported by Professor Porta in his great work on angiectiasis only five affected the genital organs, but not one had its seat on the glans. This is also the experience of Professor Sangalli, who has never seen such a tumour in his practice, in the pathological theatres, or in the numerous museums in Italy and abroad which he has visited.

(At the East London Children's Hospital, about a year and a half ago, a child was brought to me who had a nævus the size of a threepenny-piece on the left side of the glans near the meatus. The boy was two years of age, and the growth had escaped notice on account of

his long and adherent prepuce, and it was because of vesical symptoms that he was brought, the tumour was noticed when the foreskin was retracted. A couple of setons cured it.)

Case 3.—*Spasm or tenesmus of the muscular portion of the urethra and neck of bladder cured by cystotomy.*—M. Parona assisted M. Bottini a short time since when the latter surgeon performed subcutaneous incision of the neck of the bladder in a case similar to the one about to be related. Although in Bottini's case the patient did not remain long without a return of the symptoms, M. Perona determined in his case to adopt a somewhat similar plan, but to be more energetic in his treatment. In July, 1873, a young man of good constitution, æt. 22, consulted him, and stated that since a gonorrhœa contracted at the age of fifteen he had been suffering. He was advised to enter the hospital at Novare, and M. Parona recognised most of the symptoms which he had observed in Bottini's case—viz. no discharge, vesical irritation, painful contractions at neck of bladder during micturition, irregular jerky stream, urine normal and without parasites, catheterism easy in the greater part of the urethra, but possible only at the region of the neck when the resistance was tired out, bladder healthy. M. Parona arrived by the method of exclusion to the diagnosis of "spasm or tenesmus of the muscular portion of the urethra at neck of bladder." He first tried antispasmodics—bromide of potassium, belladonna rubbed into the perinæum, chloral, morphia, and then forced dilatation of the neck of the bladder—but without success. He then hesitated between intra-urethral section of the neck of the bladder and cystotomy, and decided on the latter for the following reasons:

1. In incising the neck with Mercier's instrument one runs the risk of not cutting enough of the muscular fibres and of not obtaining a permanent cure, as in Bottini's case.

2. This operation exposes the patient to the dangers of hæmorrhage, of accumulation of blood in the bladder, of infiltration of urine in the wound, and its absorption may lead to the most disastrous consequences.

3. It is difficult with Mercier's instrument to divide the membranous urethra, which participates in the alteration at the neck and necessitates the same treatment.

4. In the history of surgery one finds many cases of cystotomy followed by permanent cure. Miche-Ange Aasson relates the case of a woman who suffered from a vesical neuropathy which was taken for a vesical calculus and was cured by cystotomy. Roux relates an instance in which he did lithotomy on one of his confrères and found no stone, but the patient recovered rapidly from the operation and the symptoms vanished. The celebrated Medoro, surgeon of Padua, relates a similar case, and others are reported by Borsiori, Ucelli, Velpeau, and Dolbeau, who, in his 'Leçons de Clinique Chirurgicale,' does not hesitate to recommend this treatment for the spasmodic affections of the neck of the bladder,

arguing from the happy effects which follow incision in affections of a similar nature of the rectal sphincter. (If we mistake not, Mr. T. Holmes and Mr. Bickersteth (of Liverpool) have reported similar cases, and the latter surgeon suggested opening the bladder by the lateral method in painful bladder affections. (See 1st No. of 'Liverpool Hospital Reports.') Mr. Bryant has recently adopted this plan.

Convinced by these facts M. Parona determined on cystotomy, and on 24th October operated in the following manner:—Median incision into urethra, introduction of Dupuytren as for lithotomy, large *débridement* of the neck of the bladder. In a month the patient was well and free from his former sufferings.

M. Parona makes the following concluding remarks:

1. Palliative means, which one should always try, are in a great number of cases absolutely inefficacious.
2. The surest treatment is incision of the neck of the bladder.
3. Cystotomy should be preferred to intra-urethral section.

Case 4.—*Iodoform in the treatment of chancres.*—M. E. Besnier was the first to apply iodoform locally to chancres, and MM. Aran, d'Amico, Bazzi, Petiteau, Guenhalgh, Ferréol, Lallier, and Niezhowsky, extended its application to syphilitic and scrofulous ulcerations. The last three of these writers have specially recommended it for the treatment of fissures of the anus, "for it has the double advantage," says M. Ferréol, "of calming the acute pain of defæcation and of causing the cicatrization of the ulcer." Guenhalgh (Greenhalgh?) attributes to it an alterative and disinfecting action. M. Parona, who has tried this medicine in a large number of cases, fully confirms these observations, and it is specially in the treatment of rhagades that its treatment has seemed to him of marvellously rapid and sure effect. After having hesitated some time on the mode of its employ in these particular cases, having used alternately the crystalline powder itself or mixed in a suppository, he now gives the preference to an ointment containing a third part of it, which must be kept in contact with the ulcerated parts by means of charpie and renewed two or three times daily. Under the influence of this treatment the irritant action of the fæcal matters is neutralised, the pains disappear, reflex spasm is calmed, and cicatrization takes place very rapidly. He adds many demonstrative observations to his memoir.

Syphilitic Urethral Discharges. — In 'St. George's Hospital Reports' Mr. Henry Lee publishes a paper on this subject and states that urethral discharges may be due to syphilitic infection, and may be either primary or secondary. (Our experience fully confirms Mr. Lee's observations.) The following two cases will suffice as a description:

1. A patient some days after having exposed himself to infection noticed a greyish discharge from his urethra; it was thick and resembled a solution of arrow-root in water. There was little or no pain in micturition, little or no swelling of the meatus urinarius.

After some days the mucous membrane of the gland and the prepuce became at some parts the seat of an adhesive infiltration, which left after some days some induration. The inguinal glands became affected, and some weeks later symptoms of constitutional syphilis showed themselves.

2. A syphilitic subject, married; sexual excitement determined a slight discharge from the urethra, accompanied or not by an ulceration of the mucous membrane. This discharge, Mr. Lee thinks, is contagious and can give syphilis. Hunter had, then, reason to think that a urethral discharge could give syphilis, although he was wrong in believing that ordinary gonorrhœa was of a syphilitic nature. The paper should be read by those interested in venereal diseases.

Abdominal Section for Intussusception.—It is now a year ago since Mr. Hutchinson brought before the Medical and Chirurgical Society his case of intussusception successfully treated by abdominal section. At the time, it created a lively interest among surgeons, and now that the paper is published *in extenso* in vol. lvii of the 'Transactions' of that Society no doubt it will be carefully perused by many more. An elaborate and excellent table, containing the most noteworthy points of 132 cases, compiled by Mr. Waren Tay, is appended to the paper and will be of much value to those interested in the literature of the subject. Altogether the paper is a very valuable contribution to the practice and literature of surgery. The account of the case with the details of the operation and after-treatment have already appeared; with these, then, there is no necessity to deal here, but we wish to give a wider publicity to Mr. Hutchinson's conclusions than, perhaps, the excellent volume before us can afford.

1. That it is by no means very uncommon for intussusception to begin at the ileo-cæcal valve, and to progress to such a length that the invaginated part is within reach from the anal orifice or even extruded.

2. That it is of great importance in all cases of suspected intussusception to examine carefully by the anus.

3. That in almost all cases of intussusception in children, and probably most in adults, the diagnosis may be made certain by handling the invaginated part through the abdominal wall.

4. That the prognosis of cases of intussusception varies much—first, in ratio with the age of the patient; and secondly, with the tightness of the constriction.

5. That in a large proportion of the cases in which children under one year are the patients death must be expected within from one to six days from the commencement.

6. That in the fatal cases death is usually caused by shock or by collapse from irritation, and not by peritonitis.

7. That in many cases it is easy, by estimating the severity of the symptoms (vomiting, constipation, &c.), to form an opinion as to whether the intestine is strangulated or simply irreducible.

8. That in cases of strangulated intussusception, whilst there is great risk of speedy death, there is also some hope that gangrene may be produced and spontaneous cure result.

9. That in cases in which the intussuscepted part is incarcerated and not strangulated there is very little hope of the occurrence of gangrene, and it is probable that the patient will die, after some weeks or months, worn out by irritation and pain.

10. That the chances of successful treatment, whether by the use of bougies or by the injection of air or water, are exceedingly small, excepting in quite recent cases, and that if the surgeon does not succeed by them promptly it is not likely that he will succeed at all.

11. That the cases best suited for operation are those which have persisted for some considerable time, and in which the intestine is only incarcerated, and that these cases are also precisely those least likely to be relieved by any other method.

12. That in the cases just referred to, after failure by injections, bougies, &c., an operation is to be strongly recommended.

13. That the records of post-mortems justify the belief that, in a considerable portion of the cases referred to, the surgeon will encounter no material difficulty in effecting reduction after opening the abdomen.

14. That the circumstances which might cause difficulty are—first, the tightness of the impaction of the parts; secondly, the existence of adhesions; and thirdly, the presence of gangrene.

15. That in selecting cases suitable for operation the surgeon should be guided by the severity of the symptoms in his estimate of the tightness of the strangulation, and also as to the probability of gangrene having already set in.

16. That in cases in which the patient's symptoms are very severe, or the stage greatly advanced, it may be wiser to decline the operation and trust to the use of opiates.

17. That the operation is best performed by an incision in the median line below the umbilicus.

18. That in cases of intussusception in young infants (under one year of age) the prognosis is very desperate, scarcely any recovering excepting the few in whom injection treatment is immediately successful, whilst a large majority die very quickly.

19. That the fact just referred to may be held to justify, in the case of young infants, very early resort to the operation.

20. That it is very desirable that all who in the future have the opportunity for post-mortem examination of intussusception cases should give special attention to the question as to whether an operation would have been practicable, and should record their results.

REPORT ON PHYSIOLOGY AND HISTOLOGY.

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Blood.—Circulation.

1. C. Paquelin and L. Jolly. *Sur la Matière colorante du Sang (hématosine)*. Séance de l'Académie des Sciences de Paris, 19th October, 1874. (Reported in 'La France Médicale,' 28th October.)
2. Johannes Fürst Tarchanoff. *Ueber die Bildung von Gallen-pigment aus Blutfarbstoff.* ('Pflüger's Archiv,' 1874, p. 53.)
3. A. H. Garrod. *On some Points connected with the Circulation of the Blood.* (*Arrived at from a study of the sphygmograph trace:* in 'Proceed. Roy. Soc.,' 1874, vol. xxii, p. 291.)

1. MM. Paquelin and Jolly state that in a memoir already presented to the Academy of Sciences (10th March, 1873) they demonstrated—1, that iron exists in the red blood-corpuscles in the form of tribasic phosphate of the peroxide; 2, that the colouring matter of the blood (hematosine) does not contain iron. In the present paper they give the details of the extraction and purification of hematosine. They propose two methods for the extraction of hematosine. The *first proceeding* comprehends two operations. In the first the globules, or rather the principles forming them, are isolated from the proteic matters of the plasma; in the second operation hematosine is separated from the other elements of the globule. The separation of the globule is based on the property that subacetate of lead has of forming an insoluble combination with the albuminoid compounds of the plasma, whilst it forms no combination with the principles of the globule. The globules thus obtained are dried and pulverised. The powder mixed with five times its weight of crystallized acetic acid is macerated for five or six hours in a water bath at a temperature not exceeding 90° F. The corpuscles are then dissolved and transformed into a gelatinous mass. On cooling, a quantity of benzine or carbon disulphide, amounting to ten times that of the acetic acid employed, is added. After several days a more or less deep red fluid is obtained, which is filtered, and a fresh quantity of the benzine or carbon disulphide added till the mass is entirely decolorised. All the liquids are mixed and submitted to distillation, and a black amorphous mass remains which is hematosine still containing iron. The *second proceeding* consists in removing in the first place the colourless portions of the globule, and thus obtaining the colouring matter in as small a bulk as possible; with this object in view the dry and powdered corpuscles are macerated in alcohol (90 per cent.) containing 10 per cent. of ammonia, and after distillation to dryness acetic acid and benzine

are employed as before, but still the hematosine obtained contains iron.

In regard to the purification of hematosine the action of the acetic acid in the above proceedings leads to the formation of acetate of iron and of biphosphate of iron, and in order to remove the iron they dissolve the impure hematosine in ten times its weight of acetic acid, adding to the liquid a quantity of citric acid amounting to one fourth of the acetic acid employed. The whole being slightly warmed, some water is then added, and the whole brought to and kept at the boiling-point for fifteen minutes. On cooling, the liquor becomes cloudy by precipitation of the hematosine, and ammonia is added drop by drop to neutralise the acid. After a few days the hematosine is found at the bottom of the vessel, forming a soft resinous layer. The superjacent liquid on the addition of ammonium hydrosulphate deposits a black precipitate of iron sulphide. The hematosine dissolved in ether, when evaporation is allowed to take place, appears in the pure state as a black, shiny, and friable substance. It burns without leaving any ash, and is insoluble in pure water; it is soluble to a small extent in a dilute solution of ammonia, to which it gives a pale yellow tint. It is altered by solution of caustic potash and soda, to which it gives a brown tint. It is slightly soluble in alcohol, the solution having an amber colour. It dissolves readily in ether, chloroform, benzine, and carbon disulphide.

2. Tarchanoff attributes the different results obtained by Kuhne and Hermann on one side and Naunyn and Steiner on the other, in regard to a possibility of the biliary colouring matter being formed in the circulatory system from blood-colouring matter, partly to the fact that the former experimenters used dogs, the latter rabbits, and partly to the different mode of testing employed. He appears to agree with Hermann, and thinks that it is so formed, and that it is excreted in the kidneys, but undergoes reabsorption into the blood.

3. Mr. A. H. Garrod finds that the length of the *cardio-systole*, or interval between the commencement of the systole and the closure of the aortic valve in each cardiac revolution, is constant for any given pulse rate, and that it varies as the square root of the length of the pulse beat only. He finds also from the sphygmograph trace at the wrist that the length of the *sphygmo-systole*, or interval between the opening and closing of the aortic valve in each cardiac revolution, is constant for any given pulse rate, but varies as the cube root of the length of the pulse beat. By measurements of sphygmograph tracings from the carotid in the neck and the posterior tibial in the ankle Mr. Garrod shows that the length of the sphygmo-systole in those arteries is exactly the *same* as in the radial. The period occupied by the ventricle in elevating the pressure of the blood in its interior to that of the blood in the aorta, which may be calculated from the above, he terms the "syspasis." Its length is found to be constant for any given pulse rate, but to decrease very rapidly with increase in rapidity of the heart's action,

becoming *nil* when that reaches 170 per minute. Mr. Garrod describes a double sphygmograph for taking simultaneous tracings of two distinct vessels, as the radial and posterior tibial. The results of experiments made with this instrument show that there is an appreciable *acceleration* of the movement of the pulse wave as it gets farther from the heart.

Alimentary System.

1. Ludwig von Thanhoffer. *Beiträge zur Fettresorption und histologischen Structur der Dünndarmzotten.* ('Pflüger's Archiv f. Physiologie,' 1874, p. 391.)
2. Herbert Watney, M.A. *Note on the Minute Anatomy of the Alimentary Canal.* ('Proceed. Roy. Soc.,' vol. xxii, 1874, p. 293.)

1. Von Thanhoffer states that last winter he had experimented on certain frogs, dividing the roots of the special nerves, and that once or twice on examining the epithelium lining their duodenum he has observed cilia-form processes project and undergo retraction. These processes moved onwards blood-corpuscles or detached epithelial cells; they appeared also to effect the introduction of minute granules of fat into the interior of the cells. The cells in which these phenomena were observed for upwards of three quarters of an hour were strongly tinged with bile. He was unable to observe any such processes or movements in spring or summer frogs. In January of the present year he again saw the same phenomena, and then published a provisional communication in which he maintained that the striation of the intestinal columnar cells was attributable to protoplasmic processes projecting from the interior of the cells, and that these were of great importance in the absorption of oleaginous substances. Von Thanhoffer then describes the characters of the cells in question in minute detail. He regards the true basal border which they present as a hardened part of the edge of the cell membrane, varying in breadth and thickness in different cells even in the same villus; if a bird's-eye view be taken of the surface these hardened borders seem to be continuous with one another and to form a regular mosaic. The processes resembling hairs or cilia proceed directly from the contents of the cell, and are not due to the division or splitting up of the true basal border. He has never been able to observe any movements in the processes of the cells of mammals, and even in frogs the movement of the processes only occurred in some and not in all of the cells. It was remarkable that the bile, which arrests the movements of ordinary ciliated epithelium, seemed rather to excite these processes to activity. During the absorption of fat the processes are usually retracted. Water arrests their movements. At their attached extremities Von Thanhoffer found that they gave off two processes, of which one joined the processes of the connective-tissue-corpuscles of the matrix of the villus, whilst the other appeared by its chemical and anatomical characters to be a nerve-fibre. Further examination

showed that the nerve-fibre penetrated the cell, and that it could be followed to the nucleus. It is therefore not improbable that the columnar cells of the intestines are capable of being directly acted upon by the nervous system. Von Thanhoffer examined carefully the so-called cup-, chalice-, or goblet-cells, and believes that they are nothing more than the ordinary columnar cells modified by physiological processes. Lastly, he has been able to observe the presence of both longitudinal and transverse muscular fibre-cells in the villi.

2. Mr. Watney finds connective-tissue-corpuscles amongst the columnar epithelial cells of the intestinal tract of many animals (monkey, sheep, cat, dog, rat, rabbit). The cells are round and nucleated, and there is a delicate reticulum observable which is continuous with that formed by the most superficial layer of connective-tissue-corpuscles = basement membrane. This is the case at the pyloric end of the stomach, on the villi over Peyer's patches, and in Lieberkühn's glands. The lining endothelium of the lymph-vessels of the mucosa is in anatomical continuity with the reticulum of nucleated cells, so that it may be said the endothelial cells of the lymphatic vessels are only transformed connective-tissue-corpuscles.

In animals killed during the absorption of fat (cream) the fat can be seen in preparations stained by osmic acid as small black particles —1st, arranged in lines between or around the epithelial cells; 2ndly, in the basement membrane; 3rdly, in the connective-tissue stroma of the villus, whence it can be traced into the lymph-vessel. This indicates, he thinks, that the fat is absorbed by the processes of the connective tissue which exist between the epithelial cells and thence finds its way by the connective-tissue stroma to the lymph-vessel. The reticulum of the nucleated cells of the mucosa forms a special sheath to the vessels and unstriped muscular tissue. Mr. Watney adds a few words on the state of the mucous glands of the tongue, when at rest and during secretion.

Secretion.

1. Ch. Legros. *Sur la Structure et l'Epithelium propre des Canaux Sécréteurs de la Bile.*
2. M. E. Roux. *Des Variations dans la Quantité de Urée excrétée avec une Alimentation normale et sous l'influence du Thé et du Café.* ('Archives de Physiologie,' Nos. 4 and 5, 1874.)
3. Dr. De Sinéty. *Récherches sur les Globules du Lait.* ('Archives de Physiologie,' 1874, p. 479.)

1. M. Legros recommends for the demonstration of the biliary canals the simultaneous injection of the blood-vessels and of the biliary ducts. After having experimented on a great variety of animals he finds the liver of the rabbit best adapted for injection. The injection should be made immediately after death, and as little injury should be inflicted on the organ as possible. He warms the liver, and allows the injections to percolate for several hours through the vessels at a very low pressure. He describes the finest biliary

ducts passing between the ultimate cells of the liver as being composed of extremely *small flat cells.* He supports the view that the liver is a double gland, of which one, the *glande* or *organe biliaire* of Robin, is composed of a delicate reticulation of tubes, whilst in the meshes of this is a second organ, the glycogenic liver of Bernard, composed of the large glycogen-holding cells.

2. The results arrived at by M. E. Roux are as follows:

1. The quantities of urea, of uric acid, of chlorine, and of phosphoric acid, expelled in twenty-four hours by the urine, are very constant under the same system or conditions of alimentation.

2. As long as the amount of urine passed is the same or varies but slightly, the relation of the urea to the chlorine is constant.

3. The ingestion of a large quantity of water does not cause any augmentation in the amount of urea, uric acid, or phosphoric acid, excreted in the twenty-four hours.

4. The acidity of the urine is very notably diminished by the ingestion of a large quantity of water.

5. The quantity of chlorine injected in twenty-four hours increases with the quantity of the urine emitted, and by consequence of the amount of fluid taken.

6. In a subject who is not accustomed to the use of coffee the ingestion of this fluid augments all the solids of the urine.

7. The increase is specially marked in the case of the urea and of the chlorine. The average proportion of chlorine to the urea undergoes no alteration.

8. If coffee be commonly consumed the system becomes habituated to its use, its effects are less marked; the excretions of the urea and of the chlorine fall to their normal amount.

9. The same conclusions are applicable in regard to tea, only that the effects are less marked with tea and more transient, probably because the dose is weaker.

3. Dr. de Sinéty, from the examination of the milk in the human female and in animals, arrives at the following conclusions:

1. That in the living organism the globules of milk have no investing membrane.

2. That all the productions described as such or considered as globules of casein are secondary productions, due either to spontaneous modification of this liquid outside the body or to the action of the tests employed by different observers.

Respiration.

1. W. H. Barlow, F.R.S. *On the Pneumatic Action which accompanies the Articulation of Sounds by the Human Voice.* ('Proceedings of the Roy. Society,' April 16th, 1874, vol. xxii, No. 152.)
2. F. Riegel. *Zur Warme-regulation.* ('Virchow's Archiv,' 1874, B. lix, p. 396.)

Dr. Barlow's paper, which is illustrated by a large number of tracings, is intended to show that the articulation of the human

voice is accompanied by definite pneumatic actions, and that those actions, many of which are insensible to ordinary observation, are capable of being recorded. The principle of registration is similar to the cardiagraph of Marey, consisting in recording the vibrations of a thin membrane placed over the extremity of a trumpet into which the person experimented on speaks. A series of experiments showed that in reading 359 syllables pronounced in 86 seconds required the use of 523 cubic inches of air, or about $1\frac{1}{2}$ cubic inch of air for each syllable, and rather more than four syllables per second, including stops. Distinct articulation becomes difficult against a pressure of 2 inches of water, and Dr. Barlow could not pronounce any words against a pressure of 4 inches without considerable exertion. The tracings show an elevation, *i. e.* increased pressure, after each syllable, especially when this contains an explosive vowel. The words "Peter Piper picked a peck of pickled pepper" show this remarkably well. The difference in the action between whispered sounds and those spoken loud is not so great as might have been expected.

2. From experiments made on rabbits and dogs which had been dosed with woorara till voluntary movement was just lost and then placed in a warm chamber raised to the temperature of their body, Riegel was able to corroborate the statement made by Ackermann, that a remarkable diminution in the temperature of the body occurred when the frequency of the artificial respirations was increased. The temperature was taken in the rectum and in the inferior vena cava, but the depth of the artificial respiration remained always the same.

Muscle.

1. Engelmann, F. W. *Mikroskopische onderzockingen, &c.* (Abstracted from the Dutch in Hofmann and Schwalbe's 'Jahresberichte der Anatomie und Physiologie,' Band i, 1873, p. 500.)
2. L. Ranvier. *De quelques faits relatifs à l'histologie et à la physiologie des Muscles Striés.* ('Archives de Physiologie normal et pathologique,' 1874, vi, p. 5, and 'Comptes Rendus,' 1873, lxvii, 1030.)
3. B. Danilewsky. *Zur Physiologie der Muskel.* ('Centralblatt f. d. Med. Wiss.,' 1874, p. 721.)

1. Engelmann undertook the careful microscopical investigation of the phenomena of muscular contraction. He selected the muscles of insects because the transverse striæ stand in them at wide distances apart. He adopted Hensen's method of fixing the waves of contraction by dipping the living muscle into a solution of perosmic acid containing 0·5—2 per cent. or into alcohol. He satisfied himself that the seat of the contractility is in the doubly refracting layers. In normal conditions, and when the contraction is not too violent, the surface of the sarcolemma remains perfectly smooth. In violent contraction the sarcolemma presents transverse striæ or wrinkles. The inflections of these wrinkles correspond to

the singly refracting substance, the bulgings to the doubly refracting substance. The contents, especially the anisotropous substance, everywhere remain in contact with the sarcolemma, and the increase in thickness is obviously the greatest in the case of the anisotropal disks. The doubly refractile substance is consequently the only contractile substance. During contraction the volume of the doubly refracting substance increases, that of the singly refracting diminishes, whilst during rest the thickness of the two disks is equal in contraction. The thickness of the anisotropal disk is three or four times greater than that of the isotropal. This is only explicable on the assumption that the anisotropal disk imbibes fluid furnished to it by the isotropal disk, and which again re-enters the former during relaxation. This absorption of fluid explains the small diminution of volume which muscle undergoes during contraction. During contraction the isotropal layer becomes darker, the anisotropal brighter, so that the singly refracting substance, which in muscle at rest transmits more light than the doubly refracting, gradually becomes towards the maximum of contraction less transparent than it. In the transition stage with an intermediate amount of contraction both are equally bright. That layer which is doubly refracting in quiescent muscle remains doubly refracting during contraction, so that the change of place suggested by Merkel does not occur. From the change of form and volume of the two layers resulting from the action of indifferent desiccating agents during rest and action Engelmann concludes that the isotropal material becomes more solid during contraction, the anisotropal, with the exception of the median disk, less firm. Engelmann thinks that elongated molecules are present in the anisotropal layer, which must not be confounded either with disdiaclasts nor with sarcous elements; that these must have considerable length in comparison with their breadth (25 : 1), and that they have a strong disposition to assume the spherical form by imbibition. The process of contraction he therefore regards as essentially an imbibition phenomenon, and compares it with various phenomena of a similar character in plants.

2. Ranvier has paid especial attention to the anatomical and physiological differences existing between the pale and the dark muscles of birds, rabbits, torpedoes, and rays. The difference of colour is not dependent upon the different amount of blood these muscles receive, since if serum be injected into the vessels the white muscles become still paler, but the red preserve their colour. Ranvier states that the two forms of muscle when exposed to electrical excitation behave differently. The *red*, when excited with an interrupted electrical current, gradually and progressively shorten. When the muscle has once contracted it remains so without communicating any vibration perceptible to the hand holding the electrical forceps like that produced by ordinary muscles. When the excitation ceases the muscle gradually resumes its former length. The *white muscles*, on the other hand, when excited by the same current, suddenly contract, and as long as the excitation lasts

exhibit contractions corresponding to each interruption. When the excitation ceases they suddenly return to their original length. In one experiment the semitendinosus of the rabbit (a red muscle) was exposed for one seventh of a second to an induction current, which was interrupted 357 times in a second. The muscle at once passed into tetanus, and the apex of a myographic curve retained a uniform height throughout the period of contraction. When the same current was transmitted for the same length of time through the adductor longus (a pale muscle) the myographic curve showed as many crests as there were interruptions to the current. In another experiment, in which there were fifty-five interruptions of the current in a second, the semitendinosus was thrown into a tetanic state, whilst the adductor marked the several interruptions. This peculiarity distinguishing the two kinds of muscle is as perceptible when the muscles are excited through the nerves as when they are excited directly, and is therefore dependent on some deep-seated physiological difference. The duration of the period of latent excitation was also found by M. Ranvier to differ in the two instances. In the case of the pale muscle it amounted to one eighty-third of a second, but for the red muscle it was as long as the one eighteenth of a second, or four times longer. After death the red muscles lose their excitability much sooner than the white. In regard to the histology of the two kinds of muscle, the fibres have the same diameter in both (0·040—0·060 mm.). The transverse striæ of the pale fibres are very distinct, whilst the longitudinal striation is scarcely perceptible. In the red fibre exactly the opposite obtains. The transverse striæ do not run straight across the whole fibre, but appear as broken lines. The red fibres have many more nuclei in their interior than the white. Each pale fibre possesses from one to four flattened nuclei lying immediately beneath the sarcolemma. The red fibres have from four to nine in the same position, besides others in their interior. In the ray the red fibres are much finer than the white.

3. Danilewsky, in his essay on the respiratory processes taking place in muscles, or, in other words, the changes in the amount of oxygen absorbed and of carbonic acid eliminated, states that his experiments lead to the conclusion that a tetanised muscle, when compared with a mechanically moved muscle, excretes more carbonic acid, but absorbs less oxygen. The quantity of carbonic acid excreted by the tetanised in comparison with that of the passively moved muscle is smaller the higher the temperature, and this may be explained on the following theory :—CO_2 is produced continuously in an excised muscle because it accompanies the stiffening (*rigor mortis*) of the muscle, and it is augmented during muscular activity. The quantity of CO_2, however, which can be excreted by a given muscle is definite, and if a muscle produces more CO_2 in consequence of activity the quantity of the gas excreted during stiffening is diminished, and *vice versâ.* It is known, further, that rigor mortis sets in quicker at a high than at a low temperature. If, now, during the experiment

both muscles become quite rigid, the quantity of carbonic acid secreted by both must be equal, quite independent of the circumstance that one of them has been tetanised; the quicker, therefore, the rigor mortis can occur the nearer unity will be the quotient obtained by the division of the amount of carbonic acid the passively moved muscle produces by the amount the tetanised muscle produces. Since, now, the elevation of temperature favours the production of rigor mortis, it follows that whilst at zero the quotient above mentioned is 10·5, at 15° C. it is 2·4, and at 25° C. 1·7.

The absorption of oxygen by the active muscle is always *less than* that of the passively moved. It may be concluded, therefore, that this stands in no direct relation with the process of muscular contraction. The reason of the greater absorption of oxygen in the case of the passively moved muscle as compared with the tetanised is due to the fact that the air around the former is constantly renewed. Increased consumption of oxygen causes no proportional increase in the excretion of carbonic acid. The process which is the source of the formation of CO_2 (and of muscular force) in muscle is not apparently dependent upon coincident absorption of oxygen. It must be admitted, therefore, that the two factors of the respiration of muscle are within certain bounds independent of one another; that the muscle takes up a large store of O into its substance, where it may long remain concealed without being excreted in the form of carbonic acid. The absorption of oxygen rises with the temperature.

Nerves.

1. C. Eckhard. *Ueber den Verlauf der Nervi Erigentes innerhalb des Ruckenmärks und Gehirns.* ('Eckhard's Beiträge,' Band vii, p. 67.)
2. Fr. Darwin. *Contributions to the Anatomy of the Sympathetic Ganglia of the Bladder in their relation to the Vascular System.* ('Quart. Journ. Microscop. Sci.,' 1874, p. 109.)
3. Dr. G. Thin. *A Contribution to the Anatomy of Connective Tissue, Nerve, and Muscle, with special reference to their connection with the Lymphatic System.* ('Proceed. Roy. Soc.,' 1874, p. 515.)

1. Eckhard, in order to ascertain the course of the *nervi erigentes* in the cerebro-spinal portion of their course, applied electrical excitation to various parts of the central nervous system, both in rabbits under the toxic influence of woorara and to animals not so poisoned. He found that, besides the proper nervi erigentes there is a nerve in the rabbit corresponding to the hypogastric plexus in man, the irritation of which effects movements of the vasa deferentia. He found also that irritation applied successively in different animals to the lower cut surface of the lumbar and cervical portions of the spinal cord, even when the section of the latter was made between the atlas and occiput, produced free bleeding from the lower part of the amputated penis, such bleeding, of course, corresponding to erection. When the lower cut surface of the lumbar portion of the

spinal cord was stimulated—provided the animal was not under the influence of woorara—perhaps in consequence of the coincident irritation of vaso-motor nerve-fibres, a speedy diminution of the hæmorrhage, and in some cases even complete arrest of it occurred. This phenomenon was independent of the capability of functional activity of the nervi pudendales communes. Erection could also be called forth by excitation of the pons Varolii and of the crura cerebri at their points of entrance into the cerebrum. Eckhard draws the conclusion that the nerves inducing erection run through the pons and take their origin from some part of the cerebrum.

Generation.

1. William Roberts, M.D., of Manchester. *Studies on Biogenesis.* ('Proceed. of Roy. Soc.,' vol. xxii, p. 289.)
2. John Williams. *The Structure of the Mucous Membrane of the Uterus, and its Periodical Changes.* ('Proceed. Roy. Soc.,' 1874, vol. xxii, p. 297.)

1. Dr. Roberts's paper is divided into three sections. The first is on the sterilization by heat of organic liquids and mixtures. He finds that when beef tea or a decoction of turnip is boiled for a few minutes and afterwards preserved from extraneous contamination it passes into a state of permanent sterility. This state is characterised by loss of power to *originate* organisms with conservation of the power of *nourishing and promoting* the growth of organisms. All organic liquids and mixtures seem capable of being brought to this state by exposure to a heat of 212° F., but the length of time during which exposure to this heat is necessary to induce sterilization varies greatly according to the nature of the materials; ordinary infusions being sterilized in ten minutes, whilst milk, chopped green vegetables in water, pieces of boiled egg in water, and other mixtures, were not sterilized unless the heat was continued for twenty to forty minutes. Hay infusion was sterilized like other infusions by boiling for a few minutes, but when rendered alkaline by ammonia or liquor potassæ it was not sterilized except after an exposure to 212° F. for more than an hour; sometimes it germinated after two and once after three hours' exposure. The two factors—duration of heat and its degree—seem to be mutually compensatory, a longer exposure to a lower heat being equivalent to a shorter exposure to a higher temperature. Speaking roughly, an exposure for an hour to a heat of 212° F. appeared to be equivalent to an exposure of fifteen minutes to 228° F.).

Section 2 is devoted to the capabilities of the normal tissues and juices to generate Bacteria and Torulæ without extraneous infection. Egg albumen, blood serum, blister serum, milk, grape, orange, and tomato juice, turnip and potato tissue—these were rapidly removed from their ordinary sites and placed in sterilized tubes, and kept at temperatures varying from 60° to 90°, and the rarity with which Bacteria and Torulæ were developed when the last condition of the experiment were carefully preserved led to the conclusion that the

normal tissues of plants and animals were incapable of breeding Bacteria and Torulæ except under the stimulus of extraneous infection.

The third section is devoted to the bearing of these facts, which is that ordinary air and water contain, in addition to their proper elements, multitudes of particles capable of provoking germination. Dr. Roberts is therefore a panspermist, and he observes that were it hereafter established that bacteria and fungoid vegetations do under exceptional instances arise abiogenically, it would not overturn the panspermic theory, but would merely limit the universality of its application.

2. Dr. Williams's paper consists of observations made on the uteri of nine women who had died in different stages of the monthly period. Dr. Williams finds that menstruation appears essentially to consist, not in a congestion or a species of erection, but in growth and rapid decay of the mucous membrane. The menstrual discharge consists chiefly of blood and of the *débris* of the mucous membrane of the body of the uterus. The source of the hæmorrhage is the vessels of the body of the uterus. The mucous membrane having undergone fatty degeneration, blood becomes extravasated into its substance, then the membrane undergoes rapid disintegration and is entirely carried away with the menstrual discharge. A new mucous membrane is then developed by proliferation of the inner layer of the uterine wall, the muscular tissue producing fusiform cells, and the groups of round cells enclosed in the meshes of the muscular bundles producing the columnar epithelium of the glands.

James Dewar and John E. McKendrick. *On the Physiological Action of Light.* ('Transactions of the Roy. Soc. of Edinburgh,' vol. xxvii, p. 141.)

Dr. Dewar and Dr. McKendrick consider they have experimentally proved—1. That the impact of light on the eyes of members of the following groups of animals, viz. Mammalia, Aves, Reptilia, Amphibia, Pisces, and Crustacea, produces a variation amounting to from 3 to 10 per cent. of the normal electro-motive force existing between the retinal surface and the transverse section of the nerve. 2. That this electrical alteration may be traced into the brain. 3. That those rays that we regard as most luminous produce the largest variation. 4. That the alteration of the electrical effect with varying luminous intensity seems to follow very closely ratios given by the psycho-physical law of Fechner. 5. That the electric alteration is due to the action of light on the retinal structure itself, as it is independent of the anterior portion of the eye, eliminating, therefore, the natural supposition that the contraction of the iris might produce a similar result. 6. That it is possible by experiment to discover the physical expression of what is usually called in physiological language fatigue. And, 7. That the method employed in this research may be applied to the investigation of the special organs of other senses.

Mr. J. L. Tupper. *On the Centre of Motion in the Human Eye.* ('Proceed. Roy. Soc.,' 1874, vol. xxii, p. 429.)

Mr. Tupper proves that the centre of motion of the eye is about $\frac{9}{25}$ of an inch, instead of $\frac{14}{25}$ of an inch, behind the cornea's anterior surface.

M. Ed. v. Beneden. *The Distinct Origin of the Testis and Ovary.* ('La Revue Scientifique.')

M. Beneden remarks that it has hitherto been almost universally admitted that the testis and ovary spring from the same embryonic organ, which, becoming differentiated with the advance of growth, becomes the testis in the male and the ovary in the female. His own researches, however, made on different kinds of zoophytes, and principally upon *Hydractinia, Echinata, Clava squamata,* and *Campanularia gelatinosa,* have convinced him that the axiom of the identity of the two sexual glands has no scientific basis. The testis he finds to be derived from the external lamina or ectoderm, whilst the ovary has its *point de départ* in the internal lamina of the embryo, which is the homologue of the endoderm of zoophytes. The homology which exists between the two primordial laminæ of the embryos of Vertebrata and the two laminæ which form the bodies of Zoophytes was first recognised by Huxley. At present the form "gastrula," consisting of an ellipsoidal cavity, circumscribed by two cellular laminæ applied to one another, the endoderm and the ectoderm, has been observed not only amongst Zoophytes and Vertebrata, but also amongst Vermes, Echinodermata, Mollusks, and Arthropods—in a word, in all the great divisions of the animal kingdom with the exception of the Protozoa. These are generally monocellular, like the Gregarinadæ and Infusory Animalculæ, and never present anything comparable to the two laminæ of the metazaries. The ectoderm of Zoophytes is homologous with the external laminæ of the embryo of Vermes, Mollusks, Echinodermata, Arthropods, and Vertebrata. The endoderm has in all metazoaries the same anatomical value. For the study of the origin of the essential organs of generation M. v. Beneden selected zoophytes, in which animals the ectoderm and endoderm persist almost unaltered throughout the whole course of evolution. He finds that the ova in Hydractinia and Clava are only the modified cells of the endoderm, which the testis develops at the expense of a cellular bud which proceeds from the inner face of the ectoderm. The male organ arises from the nervous or animal layer; the female organ proceeds from the intestinal or vegetative layer, and fecundation consists in a simple mixture of ectodermic elements with a product of the endoderm. Moreover, M. v. Beneden has shown that every sporosac originally contains within itself the rudiments of an ovary and of a testis; and if this observation be associated with the researches of Waldeyer on the Vertebrata and the established fact that all animals possess an endoderm and an ectoderm, the conclusion is forced upon us that every animal is primarily hermaphrodite. In accordance, however, with the development of one or the other, the testis or ovary remains in a rudimentary condition.

REPORT ON PHYSIOLOGICAL AND PATHOLOGICAL CHEMISTRY.

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Chemistry of Food and Digestion.—The mineral or inorganic constituents of food perform two functions, or at least are found under two conditions. Those salts which are united in fairly constant proportion with the several plastic constituents, tissue materials, and combustible compounds of the body, constitute the more important section. If salts be withheld, the perfection and integrity of the organic matters is gradually destroyed, for mineral salts continue to be eliminated. When muscles are thus exhausted of their necessary complement of salts muscular exhaustion ensues, while in the nerves the excitability which first occurs is followed by paralysis of the nerve-centres. (J. Forster, in 'Zeitsch. f. Biol.,' ix, 297.)

G. Bunge (in 'Zeitsch. f. Biol.,' ix, 104) gives the results of his experiments on the functions of common salt in the animal organism. He attributes the craving for sodium chloride shown by herbivora to the excessive disproportion between the potassium and sodium salts contained in the vegetable matters on which they feed. All good analyses of cattle food show very distinctly that the quantity of potassium present is several times greater than the sodium. Now, the exhibition of potassium salts causes the elimination from the system of an increased quantity of sodium salts, probably through a decomposition of the common salt in the blood. In a single day Bunge found more than twice the normal amount of chlorine in his urine after taking during that day in three doses an amount of potassium phosphate containing 18·24 grams of K_2O. Still more effect was produced by taking the same quantity of K_2O in the form of citrate, when an amount of common salt was eliminated (in the urine of twenty-four hours) corresponding to something like half of that calculated as present in the whole blood of the body. Bunge considers that some potassium salts decrease the excretion of phosphoric acid, though all increase that of chlorine. He further remarks that the blood-corpuscles retain potassium salts especially in the form of phosphate, and that their presence in the liquor sanguinis is injurious. Man, living on a mixed vegetable and animal diet, requires common salt from mineral sources as an addition to his food, in consequence of the quantity of potassium compounds present in the vegetables which he eats.

The nature of the transformation products of starch has been very closely studied by E. Brücke ('Wien. Akad. Ber.,' (3) xv, 126). Four to five hours after a meal of boiled starch the animals to which it had been given were killed. In the stomach unaltered starch was found together with soluble starch and that variety of

dextrin (erythrodextrin) which strikes a dull-red purple tint with iodine; in the small intestine sugar was always found, but in the stomach itself mere traces of it, showing that the ptyalic ferment had done but little work, and that its action had been arrested by the acidity of the gastric juice. The soluble starch had been formed, probably, by the action of the acid of the gastric juice. The present reporter has found that one of the dextrins, probably achrodextrin, is the main product of the action of oil of vitriol (sp. gr. 1·56) upon pure cellulose when the action takes place at about 15° C. and the dissolved cellulose is, after three to six hours, thrown into cold water. Brücke regards erythrodextrin as a product of the action of lactic acid upon starch—the lactic acid itself being producible by the action of a ferment existing in the starch upon that body. But at present, it must be owned, our knowledge of the transformation of starch, and of cellulose as well, both in the body and out of it, is by no means complete.

Those of our readers who wish to study the chemistry of starch in its most recent developments should read a little brochure of 116 pages just published at Leipzig. It is a memoir by Dr. Walter Nägeli, and gives an account of the different varieties of starch, and of dextrin as produced from starch. A complete bibliography of starch renders this pamphlet most useful.

Dr. Resch ('J. Anat. und Phys.,' 1874) concludes hydrochloric acid, secreted mainly towards the cardiac extremity of the stomach, to be the free acid mainly concerned in the digestion of albuminoids. It is concluded by Von Wittich ('Pflüger's Archiv,' vii, 18) that the fundus portion, and not the pyloric, of the mucous membrane of the stomach has the power of digesting albuminoids—that is, of converting them into peptones. G. Wolffhügel (loc. cit., 188) agrees with V. Wittich and others in denyingto the pyloric glands the power of secreting pepsin, but later on the view of V. Wittich seems modified (loc. cit., viii, 444).

The chemical and physical distinctions of different albuminoids or proteids have been discussed by A. Béchamp ('Compt. Rendus,' lxxvii, 1525). His acquaintance with previous researches is obviously very limited. Both Béchamp and Dumas seem to think that they discovered for the first time in 1873-4 the existence of three albuminoids in cow's milk; the present reporter gave methods for separating and estimating them in 1870, in his 'Laboratory Guide,' 2nd ed., p. 169.

The seat of the change and breaking up of albuminoid matters in the body has been discussed by Hoppe-Seyler ('Pflüger's Archiv,' vii, 399, 1873). He concludes that this metamorphosis cannot occur in the blood nor in the lymph bathing the tissues, but in the substance of muscles and glands. Thus it is the living cells of the tissues themselves, the albuminoid constituents of which suffer continual change and oxidation by the agency of the oxygen diffused to them through the capillaries.

The changes induced by ferments of various orders, such as ptyalin, pepsin, diastase, &c., are lessened or arrested by carbolic acid and by

its higher homologue cresol. According to Peschechonow, thymol has the same powers.

According to Mathieu and Urbain, albumin solution is not coagulated by heat if all carbon dioxide have been previously withdrawn from it ('Compt. Rend.,' lxxii, 706).

C. Voit ('Zeitsch. f. Biol.,' viii, 297) has made some experiments on the alimentary value of gelatin. It is rapidly broken down in the digestive process, but is not transformed into casein nor into any kind of albuminoid matter, though it does partially hinder the metamorphosis of such complex nitrogenous bodies, and of fat also, but in a much smaller degree.

Chemistry of Blood.—The mode in which the loosely combined oxygen of hæmoglobin effects oxidation during the change of arterial into venous blood has been studied by P. Schützenberger, who concludes that the process is one of diffusion through the cell-walls of the blood-vessels. The paper will be found in the 'Comptes Rendus,' lxxviii, 971; in the same volume, page 850, an account of Béchamp's endeavours to isolate hæmoglobin is given. Further proof is needed before we can accept the substance prepared by Béchamp's process as the true unchanged colouring matter of the blood. MM. Paquelin and Joly consider the iron of the blood to exist as ferrous phosphate (loc. cit., 1579). That it is in organic union with hæmoglobin, and a constituent element, indeed, of that important substance so long as it remains unaltered, is a much more probable view. The red corpuscles, according to Boussingault ('Comptes Rendus,' lxxv, 229), contain when dry ·35 per cent. of metallic iron, an amount which is seven times as great as the iron in dry blood-fibrin. On the other hand, Hoppe-Seyler estimates the percentage of iron in hæmoglobin itself at ·43. If, as is likely, the quantity of iron in any blood is a measure of the hæmoglobin present, it becomes easy thus to estimate the latter substance. On this subject see Quinquaud (in 'Compt. Rend.,' lxxvi, 1489).

J. Hauer ('Zeitsch. f. Biol.,' viii, 567) has conducted some important inquiries as to the tissue changes brought about or increased by bloodletting. For details and results we must refer to the original memoir; but it may be here stated that the result of the author's experiments on fasting dogs was to prove that more urea was excreted in a dog after bloodletting than before; indeed, a previously decreasing daily excretion of urea was inverted.

The number of the red corpuscles in a given bulk of blood has been estimated ('Comptes Rendus,' lxxv, 1528) by M. Malassez, by means of an ingenious apparatus and method originally devised by M. Potain. Increase and decrease in the number of corpuscles may be either apparent or real, for such changes may be due to exosmose of the liquor sanguinis causing an apparent increase; to actual formation of corpuscles, as in the spleen; to destruction of the corpuscles, as in the liver, &c. The arterial blood of a rabbit was found to contain 5,000,000 red corpuscles in one cubic millimeter, and the venous blood of the same animal 5,800,000; the blood of

man contains about 4,000,000. The formation of red corpuscles in the new-born rat has been minutely investigated by E. Schäfer ('Proc. Roy. Soc.,' March 9th, 1874).

It is considered by MM. E. Mathieu and V. Urbain ('Comptes Rendus,' Sept. 14th, 1874) that the coagulation of the blood is due to carbon dioxide (CO_2). They base their theory upon two sets of observations, in one of which they found that blood after coagulation at the temperature of the body had lost about 20 per cent. of its CO_2; in the other set they noticed the incoagulability of the blood which returns from the glandular organs, particularly the kidneys, and which was found, in the case of renal venous blood, to contain 70 per cent. less CO_2 than arterial renal blood. It is argued that in the body the blood-corpuscles, by fixing the CO_2, prevent its activity as a coagulating agent.

It cannot be considered that the long and elaborate papers of A. Schmidt ('Plüger's Archiv,' vi, 413—538) clear up the difficulties concerning the coagulation of fibrin, the nature of the blood ferment and of hæmoglobin, but they afford valuable aid in this direction and are worthy attentive study.

The condition of the carbon dioxide in the blood, which was the subject of several researches noticed in our last report, has been again investigated by several physiologists. S. Setschenow ('Pflüger's Archiv,' viii, 1), M. Mathieu and V. Urbain ('Ann. Chim. Phys.,' (4) xxx, 5; (5) l, 482; Gréhant ('Comptes Rendus,' lxxvi, 233), and other workers, have all contributed fresh facts to our knowledge of this subject and of related matters (see also P. Schützenberger and P. Bert ('Compt. Rend.,' lxxvi, 440 et seq.). E. v. Gorup-Besanez ('N. Rep. Pharm.,' xxiii, 135) has examined the blood of a patient suffering from splenic leukæmia. He found formic acid to be present, together with another acid, probably propionic, and a third acid close to lactic in properties; hypoxanthine was also present in more than mere traces.

Chemistry of the Brain.—Gschleiden ('Pflüger's Archiv,' viii, 171) finds that the fresh grey matter of the brain of all the animals experimented on—horses, dogs, &c.—is always probably from lactic acid, and that the white matter is always neutral or feebly alkaline. The same difference is observed in the grey and white substance of the spinal cord. It may be here added that the nuclein of Meischer does not exist according to J. W. Müller, at all events as a homogeneous substance. According to experiments made by Petrowsky on the fresh brains of oxen, the grey substance, when dried, contains about half its weight of albumin, with a quarter of cholesterine and fats, with very small quantities of cerebrine, &c., while the dry white matter contains more cerebrine, much more than 50 per cent. of cholesterine and fats, and of albumin only a quarter of its weight.

Chemistry of Milk.—The amount of fat in human milk was found by Dr. Schukowsky, of Moscow, to average 3 per cent. in healthy women, confined within the preceding four weeks. In mothers suf-

fering from disease or insufficiency of food the fat might sink below 1 per cent. These observations add, however, nothing to our previous knowledge of the subject as derived from the admirable papers "Du Lait" by Bouchardat and Quevenne, collected in 1857. T. Brunner ('Pflüger's Archiv,' vii, 440), gives some complete analyses of human milk, but his methods do not appear trustworthy, particularly so far as regards fat and albuminoids, which he estimates below the truth.

In a long paper on the casein of milk ('J. f. prakt Chem.,' (2) vi, 1) Soxhlet discusses the question of the identity of this, the chief albuminoid of milk, with "alkali-albuminate," a position which he endeavours to maintain by a series of experiments which at all events throw some light on the relation of phosphates to albuminoids, a relation which is general and ultimate. Soxhlet's views are controverted by Heintz (loc. cit., 374).

We are indebted to O. Jacobsen ('Deutsch. Chem. Gesell. Ber.,' vi, 1026) for a new analysis of human bile. It was a clear, dark, yellowish-green, perfectly neutral fluid, having a specific gravity of 1·0105, and giving 2·24 per cent. of fixed residue. It contained neither grape-sugar nor urea, but bilirubin and biliverdin were present. Common salt constituted two thirds of its ash, in which only 4 per cent. of potassium chloride and $4\frac{1}{2}$ per cent. bone earth occurred, besides 27 per cent. of sodium salts of carbonic and phosphoric acids; a trace of copper was found in the ash. The dry matter of the bile contained of organic matters—cholesterine 2·5 per cent., sodium glychocholate 45 per cent., with lecithin and sodium salts of three fat acids.

Chemistry of Bile.—The sodium salts of the bile acids (glycocholic and taurocholic), when injected into animals, did not produce jaundice, though vomiting, depression of the temperature and pulse, and, in some instances, convulsions occurred. Albumin was found in the urine, but no bile acids.

Chemistry of Urine.—Another new substance has been found in urine by F. Baumstark ('Liebig's Ann. der Ch. und Ph.,' 1874). It is much like hippuric acid in the form of its crystals and in its free solubility in boiling water and sparing solubility in cold, but it is a basic, not an acid substance. Its chemical composition is expressed by the formula $C_3H_8N_2O$.

Minute traces of bile acids have been found by Vogel and Dragendorff ('Zeitsch. f. Anal. Chem.,' xi, 467) in healthy urine.

The amount of ammonia in the urine of 200 persons has been carefully estimated by Dr. Tidy and Mr. W. B. Woodman ('Proc. R. Soc. Lond.,' xx, 362). The quantity voided in twenty-four hours is about two and a half grains on an average, but it is not a constant quantity even in health, and is liable to excessive variation in disease, being much reduced in amount in most febrile and complicated disorders, but increased in rheumatic gout and diabetes.

The acid reaction of urine is regarded by J. Donath ('J. f. prakt.

Chem.,' (2) ix, 172) as arising from the formation of an acid phosphate of sodium by the action of hippuric or other organic acid of the urine upon the ordinary alkaline phosphate present.

The substance called indol, C_8H_7N, an indigo derivative, if subcutaneously injected, appears in the urine as indican ('Zeitsch. f. Anal. Chem.,' xi, 358).

A. Béchamp ('Comptes Rendus,' lxxv, 1880) finds traces of physiologically formed alcohol in the urine of elderly persons not using alcoholic liquor in any form.

The amount of urea daily excreted is lowered about 7 per cent. by fifteen grams of tea, and about 14 per cent. by fifteen grams of green coffee (see Rabuteau, 'Comptes Rendus,' lxxvii, 489).

The Excretion of Carbonic Acid.—While by far the greater part of the CO_2 produced in the human organism is discharged by the lungs, yet a small quantity, though not a constant amount, is excreted by the skin. In twenty-four hours the carbon dioxide passed into the atmosphere from the latter source amounts to something like four grams, while that from the lungs is about 225 times as much, or 900 grams (H. Auber, in 'Pflüger's Archiv,' vi, 539).

Physiological Chemistry of Chloral and Alcohol.—The anæsthesia produced by the injection of chloral into a vein has been the subject of a recent communication to the French Academy of Sciences. Seven and a half grams (about two drachms) in all were injected, complete insensibility in the patient occurring in one hour forty minutes and lasting two hours. The return to a normal state required two days. The action of injected chloral is attributed by some physiologists to the carbon monoxide which it disengages during decomposition in the alkaline blood, and which gas interferes with the proper functions of the hæmoglobin in carrying oxygen.

Alcohol, according to Prof. Binz ('J. Anat. and Phys.,' May, 1874), produces a subjective sensation of warmth, but a real lowering of temperature. This is in accord with its tendency to diminish change of tissue and consequently the production of urea and carbon dioxide. The fall of temperature is only noticeable when alcohol is not habitually taken by the subject of the experiment, and has not been taken for some time before the exhibition of it for the purpose of experiment.

Toxicological Chemistry.—The poison of several snakes, *Naja tripudians* and *Daboia Russellii* more particularly, has been investigated by Drs. T. L. Brunton and Fayrer ('Proc. Roy. Soc.,' 1874). The experiments have been partly chemical and partly physiological, and have given very interesting and decisive results. From these may be selected the following facts. The poison of a venomous snake is generally incapable of affecting itself or other venomous species, but it usually proves fatal to harmless kinds. Snake-poisons are not rendered inactive by drying, while treatment with alcohol

dissolves the active part of the venom without destroying its toxic power. The symptoms produced in dogs and rabbits by the exhibition of snake-poisons are nausea and twitchings, followed by depression and drowsiness; as the effect of the poison increases paralysis sets in, power of co-ordination and motion being lost before that of sensation. Snake-poison is most virulent when directly injected into the circulatory system. The subsequent injection of ammonia is not regarded by the authors of the memoir as an antidote, nor were they able to trace any apparent changes in the corpuscles of the blood due to the poison.

The subcutaneous injection of ammonia is not considered successful as an antidote to the poison of the viper by M. le Roy de Méricourt ('Comptes Rendus,' Juin 23, 1874), nor by some medical practitioners in the East Indies, who have also tried it in the case of natives bitten by Indian poisonous snakes.

The nature of the poisoning of animals by carbon dioxide caused by increased barometric pressure has been studied by P. Bert ('Compt. Rend.,' lxxvi, 1276). The tissues and fluids of the blood become highly charged with the gas, although the arterial blood may still retain as much as 12 volumes of oxygen to 100 of blood. As internal oxidation-processes are stopped the temperature of the body is greatly lowered.

Both potassium and calcium salts, when injected into the veins, produced similar toxic effects upon dogs. MM. Rabuteau and Ducoudray consider ('Compt. Rend.,' lxxvi, 349) that the salts of these metals and of all others, save sodium and lithium, act as muscle-poisons, and so cause death by stopping the heart's action.

Iodic acid, in the form of its potassium salt (KIO_3), is poisonous, but becomes reduced in the organism to potassium iodide (see Melsens, in 'Ann. Chim. Phys.,' (4) xxv, 157).

When *all* the hydrogen in ammonium is replaced by methyl or ethyl, &c., then the salts of the new base are found to act, not as muscle-poisons, but by paralysing the motor nerves like woorara poison. On the other hand, such a salt as amylammonium chloride, in which one atom of hydrogen is replaced by amyl, has a power of lowering the pulse, and has been given with advantage by M. Dujardin-Beaumetz in typhoid fever. A dose of seven to fifteen grains lowered the pulse by ten to twenty beats per minute. It is much more poisonous than trimethylamine salts.

Professor Chauveau has made numerous experiments tending to show that tuberculosis may be transmitted by means of food containing the infectious matter. Of 100 calves so fed all exhibited the disease in two months, while on the same food, minus the infectious matter, 100 exactly similar calves remained perfectly healthy.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1875.

Analytical and Critical Reviews.

I.—The Vivisection Controversy.[1]

I.

What strange things are done in the name of humanity has been well illustrated during the past months in the controversy which has been carried on in relation to the subject of experimentation on living animals for the purposes of scientific investigation. A small section of men of science, belonging chiefly to the ranks of medicine, struck with the sufferings they witness in their own species, or straining to get a clearer view of the nature of life, are led to make use of living animals of lower species for the purpose of experimental research; for the purpose of discovering, by such research, many problems of living action; for the purpose of determining whether by such process of discovery something may not be directly or indirectly learned that shall tend ultimately to relieve or cure or remove every unnecessary pain of the world. Another and much wider section of men, to whom are added many women also, struck with what they consider to be the cruelty of the experimentalists, and blind to or careless of the severest sufferings of human-kind, are horror stricken with the sight of the wounded lower animal and are charged with disgust for the perpetrators of what they consider to be useless and abominable outrages on that mercy which should extend from man to the lowest thing that lives.

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[1] 1. *Experimentation on Animals as a Means of Knowledge in Physiology, Pathology and Practical Medicine.* By J. C. Dalton, M.D. New York, 1875.

2. *Various leading articles and letters in the public papers, on the subject of vivisection,* 1874-5.

3. *A Pamphlet descriptive of Physiological Investigations by Experiments on Living Animals.* London, 1875.

4. *Report of the British Association for the Advancement of Science for* 1871. *London,* 1872.

We have used the word "controversy" in speaking of the question before us, but in truth the term is hardly fair unless we choose to commit the bull that the controversy has been all on one side. For with one or two notable exceptions, the medical scientists who have been most abused have been singularly firm in their silence. Absorbed, probably, in their studies, they have not read what has been said of them or have condescended merely to glance at it as a fanatic storm, loud and fierce, but fitful and evanescent; a storm which the common sense and common interests of men in general will be sure to allay, if they but leave it to that nature whom they follow in her apparently relentless courses for the universal good. Thus the opponents of experimentation have, on the whole, had more than a fair field. They have said anything and everything they liked without obstruction. Their very imaginings, pitched in deepest gloom, have met with no systematic contradiction. One author only, he whose work heads the list at the commencement of this article, has written a small book in defence of his class; and that book is the coldest modesty, a calm statement of simple facts, short, to the point and earnest, but with little fire and no smoke.

Thus the two sections stand arrayed before the world: the cold, determined, silent experimentalists, who are accused of cutting up living animals; and the hot, impassioned, loud opponents of experimentation, who in their zeal are cutting up the experimentalists.

As they face each other at this present moment, we may with advantage note the characteristic signs of these two sections of modern society.

II.

The opponents of experimentation, with whom, as the aggressive body, we commence, are a mixed multitude, the members of which are influenced by widely different motives and intentions. It is an utter mistake to suppose that because on one point of their human nature they lean to mercy they are therefore merciful.

Whatever view we may hold as to the right or wrong of experimentation, we would fain admire the intentions and desires of those who oppose it, if their intentions and desires were clothed in the attributes of a mercy that was not limited to one part of the creation, but was universal in its sympathy. Unfortunately the picture they present is not of this kind; it has a ray of sunshine on it, lighting it from one side, but it has also a side very dark, darkened, strange as it may seem, even by cruelty. The whole of the sympathies they express are poured forth for the dumb creatures they have taken under their pro-

tection. For the objects for which the lower creatures are sacrificed by men of science not a thought is permitted: for the useful results that have sprung from such sacrifice not a syllable is allowed. This, perchance, is only negative severity; but the positive is not far distant. For the feelings of those gentlemen who in the pursuit of knowledge have felt it their duty to follow the experimental method on living animals no respect is paid. On the contrary, some of them are treated with a vengeance that knows no weariness. What the extreme opponents of experimentation who are so callous to the infliction of mental suffering would do if the law gave them power to add the infliction of corporeal suffering upon those who are opposed to them, it were terrible to say. In their blind zeal the treadwheel and the lash were certain additions to the means of repression they would aim to administer. Perchance they would introduce the revival of the lost art of homisection for the punishment of science, and would restore the glories of the chamber whence the "e pur si muove" had its immortal birth. Who knows, then, but that with the advance of civilization and humanity, a man too earnest in the pursuit of some of the grandest problems the mind can follow might be sacrificed for a rat.

We notice, moreover, amongst the zealous opponents of experimentation some rather startling inconsistencies, the study of which leads us to speculate as to what is the true depth of their tender-heartedness even to dumb brutes. Some of them in a rude way, and without the slightest remembrance that there are such admirable agents as anæsthetics at command for the use of the humane experimentalist do themselves take part in or sanction for their own pleasures or interests various of the most painful experiments on living dumb animals. In humble imitation of the mode in which the experiments of the vivisectionists are branded and handed about, we will give a few illustrations of another kind in which the opponents of vivisection are fully privileged to indulge.

Experiment 1. *Artificial production of death by tetanus.*—A common experiment, performed by more than one of the privileged section, is the following. It has the advantage of being made without any object of benefit either to the human or to the lower animal. The operator prepares himself for this experiment by putting upon each of his boots an instrument containing a revolving wheel which is armed with a number of very sharp needles. Thus prepared he mounts the back of the inferior animal known as a horse, and rides to a place that is technically described as "a meet." Here he mounts another horse that has preceded him. From this place he proceeds to

a "cover," where he finds a great number of dogs. The dogs are driven into the cover, and after a time there is seen escaping from it a beautiful dumb animal named a fox. The fox flies across the fields pursued by the dogs. The expression of its awful terror beggars our description, but still it is pursued, and the operator, who has come for the sole purpose, joins in the chase. As his horse flies along he explains why he has armed his boots with the revolving spikes. He plunges these spikes into the delicate sides of his horse to make it go even beyond its powers. As the sharp needles enter the sides of the animal the phenomena of reflex action are strikingly illustrated. The impression made by the needles on the sensitive peripheral terminations of the nerves derived from the dorsal and lumbar portions of the spinal cord, after being conveyed to their centres, are reflected upon the external abdominal muscles, causing sharp muscular twitches which are purely involuntary. Sometimes a little blood is lost in this experiment, but as a rule the wounds heal favorably and the animal is not materially injured. Meanwhile the fox, still pursued, after a run of some miles, rushes into a hole in the ground. He is then said to have run to earth, and if he cannot be reached by the dogs he is left there. By the chase to which he has been subjected there is often induced in him the disease now defined as heat tetanus. It consists of a peculiar and excruciating rigidity of the muscles, like that which follows upon the administration of strychnine. The tetanic spasm alternates with relaxation, but the condition is fatal. The tetanus may continue for so long a time as three days. After death, from an animal thus run to death, the body is in a state of rigid spasm. The death has taken place, indeed, from a general tetanic convulsion, in which the muscles of respiration have become involved.

Experiment 2. Production of complete muscular exhaustion, followed by death by the tearing of limb from limb.—The above experiment is usually considered a failure, and is followed, therefore, by another, in which all the steps of the operation are the same except the last. In this case the dumb animal, the fox, finding no hole in which to run, is hunted down by the dogs. The dogs seize the animal by different parts; one tears off a limb; another gnaws a large piece out of the side; a third tears away at the head, and a human assistant, lashing right and left at the dogs, makes a grab at the tail, which he commonly secures and presents to a lady if there be one present. The experimentalist himself takes an enthusiastic part in this division of the experiment; he regrets only that the death, though it is terribly severe, is rather short, and he follows a rule, as a finale, of raising himself a little in his saddle, at the

moment the tail of the fox is secured, and of shouting as loud as he can in signification, we believe, that the experiment is considered a success. One gentleman we know who delights greatly in this mode of animal experimentation bases his chief argument against the vivisectionists on the statement that they repeat, uselessly, several times painful physiological experiments. The experiments of the objector above detailed, which are quite as severe as any known from physiological research and are carried on without anæsthetics to the bitter end, have, it is fairly computed, been repeated by him five hundred and eighty-three times. We need hardly say that the results have been entirely personal. They have ministered to the pleasure and happiness of the operator and his co-operators, and that is all. If any physiological experimentalist had subjected five hundred and eighty-three foxes to an experiment with the object of discovering or elucidating some great truth in science, however humanely he had carried out his research, he would be received with unmitigated execration. In his case it would have signified nothing how slight the experiment was, though it were the mere opening of a vein, nor how useful the intention. He would be subjected to the penalties of insult, degradation, and, if possible, of trial and absolute ruin.

Experiment 3. Severe irritation of the peripheral nerves of the skin in a dog for the purpose of destroying an instinctive faculty.—A very determinate opponent of animal experimentation is fond of dogs, and is horrified that they should ever be used by the physiologist. He has performed himself the following experiments. He has dogs which have the ability of pointing out in the fields the position of game. They are exceedingly intelligent and sensitive animals. In learning their art they sometimes, before the sportsman has fired, or while he is firing, run after the game. To break them of this instinctive faculty our experimentalist grasps the animal firmly by the back of the neck, and with a short whip, called "a dog whip," lashes it within an inch of its life. The screams of the unfortunate animal as it bears the torture are fearful to listen to, but they avail nothing. In some instances the animal, having inadvertently committed the instinctive offence a second time, is conscious, when he is called off, of the torture that is to follow. Then he approaches his master with penitent and imploring look, and even licks the hand that is about to smite him. The signs of obedience and affection are unheeded, the instinctive faculty must be cut out; again the instrument of torture is plied, and the screaming animal yells forth his agony and despair. At last, probably after a repetition of the process, the experiment succeeds.

The same result has been tried for by another process, viz. by lodging a charge of shot in the dog when he is at long range. The experiment may be successful, but there is the danger of it that the creature may be killed outright or seriously injured. It is therefore less frequently resorted to than the first experiment.

The experimental gentleman in this department of mercy is particularly indignant that the physiological experimentalists should be such brutes as to use dogs for their researches. He would like in any new legislation on that matter to give the magistrates power to inflict something more than a fine on any physiologist who should be caught experimenting on his favourite animal. He thinks a month's imprisonment and twenty lashes should be the peremptory penalty on every clear conviction, and he wishes that he could not only direct but lay on the chastisement.

Experiment 4. *Mode of producing death in a rabbit by paralysis of the hinder limbs and starvation.*—A gentleman who is a staunch opponent of animal experimentation was walking out with his gun in the evening time. On the edge of a rock there sat a rabbit that had just left her litter of young in a snug bed in a small adjoining warren. Hearing a noise, she essayed to leap from the rock, but was too late. The experimentalist with gunpowder and metallic shot had deliberately lodged a charge from his experimental tube into her loins. She fell down the rocks into a ravine near to where her young ones were concealed, and there she lay with her hinder limbs paralysed and one fore paw fractured by the fall. She was unable to move, even to the short distance where her young were nested. The pain in her injured limbs was excruciating, and to this was soon added the extreme agony of tension from the collection of milk in her mammary glands; but her great pain was that she could not reach her little ones to tend them and give them their food. Alas! poor animal, they were all dead from starvation long before she ceased to think of them and yearn for them. After some days of continued pain she became unconscious from want of sustenance, and at last, like her brood, died of starvation. The experimentalist who perpetrated this experiment, unable to find his prey or too careless to seek long for it, returned to dinner, and after enjoying consumedly some pâté de fois gras retired to his study and informed his friends of his willingness to pay his quota to the expenses required to try to secure a conviction of any miscreant physiologist who may be caught performing any operation whatever for any purpose whatever on a living animal.

Experiment 5. *Production of gyratory movements in the pigeon by an injury to the nervous centres.*—Another opponent of vivisection is fond of experimenting on pigeons by introducing forcibly into their bodies while they are flying small particles of lead. Sometimes the foreign bodies thus introduced strike a vital point, and the animal falls to the ground senseless or dead; but as the experiments are perfectly objectless and are made at random, they as frequently miss this fatal mark as hit it. In one case the experimentalist shot at a pigeon, which was observed in falling to make a number of gyrations and somersaults. When it reached the ground it rolled over and over again like a football and thus escaped. The phenomenon was much talked of afterwards by this gentleman as one of "the strangest things he had ever heard of in his life." If he had seen the end he would have known that the bird, owing to the nervous injury it had received, continued to carry on those strange rolling movements for nearly a week before it was fixed in death. He has had some other curious results of his experiments. Once a shot went into one eye of a bird and out of the other without killing the animal, and he kept it for some weeks as a curiosity. He thinks that the great evil of physiological experiment is that the fellows who lend themselves to it become such hardened brutes and set a bad example. He would stop them on that ground alone, but fancies it is, perhaps, a sufficient punishment to hold them up to public scorn and reprobation. His heart bleeds for the victims of their atrocious cruelty.

Experiment 6. *Experiment for destroying the procreative faculties of animals by cutting and forcibly tearing away the glands.*—This is an experiment largely carried out by many who are strongly opposed to vivisection. It is performed without the use of any anæsthetic, and often with disgusting details. It is an experiment horribly painful. One gentleman who belongs to the class of reformers now under consideration has it done wholesale on the lambs he breeds on his estate. He has the little creatures brought up to a table and held down with their fore and hind limbs extended; in simple language they are crucified. His assistant then cuts off from the scrotum of the lamb a portion of skin and forces one of the glands partly through the opening; then he seizes the gland between his teeth and tears it out by main force. The experiment is next repeated on the other side, and the success is considered certain. Before the animal leaves the operating table its tail is severed into two parts by an incision through it, with either fracture or dislocation of the bone. The mutilated victim, bleeding and

faint, is now set at liberty, but is often tormented for many days by flies, which attack the wounded surfaces. This same operation with the same avoidance of anæsthesia is also performed on other animals belonging to this eminent philanthropist, viz. dogs, cats, horses, and pigs. He is himself interested in the experiment and will spend some hours when the lambs are being crucified and experimented upon, watching with admiration the skill of his manipulator. But he is so fearfully disgusted with the French physiologist, Dr. Magnan, who "crucified" two dogs at Norwich, and injected absinthe and alcohol into the veins, that he calls this eminent foreigner "an incarnate French devil." He is disgusted with Mr. Colam, for not catching the reprobate in London, and, indeed, thinks the whole case was so shamefully botched that another society than that represented by the able and energetic officer named is urgently needed. He too would put a stop to physiological experiment of every kind and by every person. He believes the talk about the physiologists doing their work for the public good is all humbug. They do it for their own interests, and care no more for humanity than for the poor and helpless dumb animals that come into their hands.

Experiment 7. Method of transfixing an animal on a barbed point; test of the voluntary muscular power of a cold-blooded animal. Production of slow death by asphyxia, with marked evidence of a convulsive stage.—The experimentalist in this instance selects the large fish called a pike for his experiment. He prepares a sharp barbed piece of steel bent into the form of a hook. He conceals the hook with some food which the pike likes, and by the treacherous and cowardly method of laying this food in the path of his victim he induces the animal to swallow the barbed hook. So soon as he feels the creature has gulped the hook he drags the sharp and barbed fangs through the soft and delicate tissues, not heeding how far they pierce or what suffering they inflict. The fish, feeling itself thus transfixed, struggles to get away from its tormentor, and now comes an exciting part of the experiment. The fish is at one end of the line, the philanthropist at the other. The fish in its throes pulls, the philanthropist pulls, or gives a little line and then pulls again; thus, the test of the muscular power of the cold-blooded animal is put to the severest and most awful trial, until, this muscular power failing, the philanthrophist conquers and brings his prey out of its watery element to dry land. Brought into an element in which it cannot breathe, the symptoms of asphyxia are developed in the fish. It becomes actively convulsed, and after a struggle of several minutes dies. The experimentalist who follows out this interesting method of

experiment usually becomes an enthusiast in the practice, and resorts to it in leisure hours as a relief or pleasure from the severer duties of life. One of them who has performed the experiment we have described four hundred and sixteen times, being very greatly shocked by the news he received at his favourite fishing place last year of the barbarities of the vivisectionists, was good enough as he pursued his own experimental labours to draw up, in his mind, the clauses of a stringent parliamentary act for the suppression of vivisection. If any legislation on the matter should be attempted, it is probable that the measure conceived under these happy and appropriate circumstances will be submitted to the nation.

Experiment 8. *Effect of subjecting an animal to the action of water at a temperature of 212° F. Changes of colour produced. Singular noise emitted by the animal.*—A married lady who is most anxious to punish vivisectionists and all other experimenters on living animals has performed for her the following experiment. An animal called a lobster is taken while fresh and alive and is placed in water over a fire. The temperature of the water is then raised to 212° F., that is to say, boiling-point. As the animal begins to suffer it gives forth a peculiar noise, which is euphoniously called by the operators "singing," but which would be considered, if the experiment were performed by a physiologist, to indicate the fact of excruciating torture, and would possibly be designated a shriek or cry. The colour of the outer covering of the animal is changed from blue black to a bright red. Its eyes, which project as if on a stalk, are put out and destroyed by the heat of the water, and its flesh is cooked as it is killed. On a very interesting occasion this lady felt she could eat nothing but lobster, and her taste for this food has ever since continued active. As that is nearly forty years ago, the incredible fact must be told that the experiment we have described has been performed for this lady alone two thousand and eighty times—probably the longest series of instances of experimental procedure by a single experimentalist on record. This estimable abhorress of vivisectionist practices thinks that the monthly publication of the names and addresses of the creatures who, under the name of science, torture dumb animals, would be the best means of checking their revolting cruelties.

Experiment 9. *Process of removing the cutaneous envelope from a living animal, vulgarly called flaying alive. Section of the muscles of the animal in many places previous to death. Contraction or crimping of the severed muscles in parts or sections.*—This is a very common experiment, performed, probably, for all the ladies and gentlemen who are opposed to

vivisection. The animals selected are usually the skate and the eel. The skate is kept as lively as is possible up to the time of the experiment, for the reason that its muscles, if they were dead, would not crimp in the perfect manner desired by those for whom the experiment is carried out. The animal is first carefully stripped of its skin, and its muscles are then divided transversely by a sharp knife in an immense number of places. As each part is severed it contracts almost as if it were a separate muscle. The quivering of the muscle continues for some time after the operation is completed, until indeed the muscular fibre, set gradually out like a huge frill, dies.

In the case of the live eel it often happens that the operaration of skinning its body is performed in the kitchen, or at the door of the house of the lady or gentleman who wishes for it. The operator in this instance asks for a handful of salt. He takes the salt in his left hand, and seizes the head of the animal firmly. The salt is used to give the operator a firm gripe, otherwise the animal wriggles so fiercely as the skinning progresses it is hard to hold it. After the animal is firmly gripped in the hand, charged with the salt, it is deliberately denuded of its skin, despite its "wriggling." It is finally hacked into transverse pieces while its muscles are still quivering.

The ladies and gentlemen who have these experiments performed for their pleasures seem to forget that they are outvying all the vivisectionists living, in the refinement of their cruelties. To what degree their inconsistency shines forth is best told in the following faithful narrative. A lady who is ardently opposed to vivisection, and who would like to have the letter V branded on the right hand of every vivisectionist, was about to have a dinner party of ladies and gentlemen of her own opinion. The night preceding this great occasion she ordered of her intelligent fishmonger a crimped skate, and in the afternoon of the important day was astounded by the announcement of the cook that the fishmonger, Mr. Donald (his further name we have pledged ourselves not to divulge), had failed to send in the fish. In dudgeon our philanthropic lady, whose impulses were always ahead of her reason, whipped on her bonnet and away to the man of the marble slab and cold-blooded viands. She found him putting the finishing touch to the skate, and, having first rated her experimentalist for his delay, went on, "peppery like," as Donald noted, to find fault with his work. Donald, of course laid the delay on the market, and as to the fault in the crimping he urged that if he couldn't get the animals until they had hardly a drop of life in their bodies, how could they be expected to crimp like

real live ones? The lady, convinced by Donald's arguments, or fearing further delay, calmed down, and, begging the fish might be sent round immediately, was about to leave the shop, when her eye caught sight of a small moving object on the side counter. "Why, Mr. Donald," she exclaimed, "what is this: it is moving like a watch?" "That, madam," replies Donald, "is the heart of the skate; it's laid aside for a medical gentleman wha' studies the heart, to see how long it will just throb." The effect on the lady was electrical. "A vivisectionist!" she almost screamed, "and you, Donald, minister to his barbarities?" Donald stood aghast; he expected next moment to hear the request that he might send in his bill. But a new light seemed to fill his visitor's face; she spoke persuasively, yet decidedly, "Donald," she asked, "who is this wretch? We may now establish a case." Donald was on guard; he was between Scilla and Charybdis, for the doctor was as good a customer as the philanthropic lady; but he was equal to the occasion. "Madam," he replied, "I ken naething o' the gentleman mair than he's student-like, grave and serious. When he ca's to-day for the heart, if I dinna mind his name and address, I'll e'en ask him." "Do, Mr. Donald, pray do," she answered, and seeing a hansom cab passing the door hailed it instanter. Donald went to open the folding doors of the vehicle for her, and heard the conversation with the cabman. "Drive me quickly to the R.S.P.C.A." "To the Arispiciary," says the driver, spelling it to himself. Then down into the cab, "Is it the new resterong at 'Ighbury, mum?"

"No, it's the animal place; take me, take me to Mr. Colam!"

Cabby, who thought he now saw his way,—down again into the cab, "At the Zoo, mum, I suppose."

"The Zoo! No, though I believe that's a bad place enough. Jermyn Street."

And away as fast as a wearied horse,—keeping a hasty lady from friends expected at dinner,—could speed under influence of jerk of rein and cut of whip, away she sped.

Meanwhile, Donald, surmising that his shop might by-and-by be under uncomfortable surveillance, picked up the still beating skate's heart, put it into a gallipot, and away himself with it to his patron the doctor. Could he see the doctor for just a second? He could, and was shown straight into the study. The man of science was busy fitting up a reoscope when Donald entered.

"I brought ye, sir, the skate's heart; it seemed to me to be stopping a wee, so I brought it mysel'." "Thank you much," answered the doctor, taking his treasure and watching the motion. "It is a strange phenomenon."

"Mair strange than ye yerself ken, doctor," said Donald, with a knowing look.

"All God's works are, Mr Donald," was the solemn reply of the philosopher. "Thank you again, and good day."

And Donald, thinking he had done enough, withdrew. He had saved his patron from being possibly followed by a detective, which was what he wanted.

Whether the lady carried out her mission of mercy to the R.S.P.C.A. we cannot tell; but certain it is that at her select party, after she had carved up and feasted her visitors upon the animal that had been beflayed and vivisected for her, and had laid a little fault respecting the deficient crispness of the animal on the fishmonger, she explained generally, with tears in her eyes, that she was on the actual track of a wretch who for the pursuit of science cut out the heart itself of living animals, and pursued his researches for hours afterwards on the quivering flesh. That fact she knew, any way, of her own direct knowledge. Her friends, awfully shocked, improved this knowledge. One friend, by looking up the 'Lancet' to see what physicians were studying the heart, made "a shrewd guess" at the right person, and tacked the assumed barbarity on the back of a gentleman who never in his whole life touched an animal with a scalpel and couldn't if he would. Another, not having remembered to what animal the lady had referred, assumed it was a dog: the dog established, it was raised to a pet dog, which licked its cruel master's hand as he was accustomed to show its poor bleeding heart to the students of his class, who shouted with wild delight at each exhibition. Finally, to complete the picture, the dog died in torments while its master, for the sake of economy, was using him for a new vivisection.

III.

We have put forward in these experimental records a few of the eccentricities and inconsistencies of those who are vehement against scientific experimentalists. Good as their motives may be, they are, it will be seen, themselves guilty of much cruelty for useless purposes. The physiologist who subjects an animal even to pain has some object in view, present or ulterior, of service to mankind, and to lower species than man. But the persons who tear foxes limb from limb in savage glee,—who torture natural instincts out of dogs by the lash,—who plant sufficient shot in game to wound mortally and not to kill, leaving their victims to die of wounds and starvation,—who have herds upon herds of young animals subjected without anæthesia to one of the most exquisitely painful operations in

barbaric art,—who catch living animals on barbed hooks and feel the death throes as so many joyous thrills through their own bodies,—who have living animals boiled alive,—who have animals skinned living and hacked into living shreds,—these have nothing to answer for but wild and wanton cruelty. The last they, who being without sin, can throw the first stone at the pioneers of knowledge. Still more unfortunately, these opponents of vivisection do not rest with their flagrant inconsistencies; they plunge into other vices. To make their arguments good they catch up every suggestion, however false and foolish it may be, and parade it as a bait to catch the simple and good-hearted unwary. In some of our London schools of medicine the students are taught to operate on the eyeball for cataract by the plan of fixing the eye of a dead animal—a pig or a sheep—into a mask shaped like a human face. Straightway the announcement of this simple procedure is paraded as a proof that students are allowed to experiment on the eyes of living animals, and although the shameless blunder is afterwards exposed and retracted, the effect it produced has never been removed. Such statements, indeed, are never removed, neither by exposure nor retractation; fixed on the minds of those who love to believe them, there they remain, as this does, in the minds of thousands. Against physiologists, whose names, places of residence, times of experiment and companions, are all suppressed, charges are made on evidences that are utterly worthless. Against the behaviour of unknown students in unknown localities similar and equally ungrounded charges are affirmed without even the pretence of evidence. Thus, if we sit down, as we confess we have sat, to consider whether some of the cruelties mentioned could by wise course of action be stayed, we find the attempt hopeless, because the charges themselves, like the mirage, bear no inquiry. As they are approached they fade into obscurity. We are not in the secrets of the Royal Society for the Prevention of Cruelty to Animals, and all we know of the society is that its action so far, as tolerant and wise as fanaticism will let it, has been one of singular difficulty and usefulness: but we will undertake to say that deluged with such mad and false statements it can take no step without fear of falling into some error that were equal to its perdition in the public confidence.

One more trait, of those who oppose experimentation on living animals for the sake of science, is most pitiable and censurable of all. They urge that the feelings of men of science are blunted by the sight of suffering in animals. They forget how much more their own are blunted to the sufferings of the human family. "Let them die, then," was

the actual reply of one of those philanthropists to a physician who was explaining that to discover a means of saving human beings from one always fatal disorder, experiment was necessary; "Let them die, then, rather than a single animal be subjected to experiment." Nor was this objector alone; there are numbers of belated sentimentalists who share the same infatuation, until they themselves feel pain or see their young and beloved ones carried away. Then they are the first to deride our medical art, and to tell us, when they have crippled us in our researches, that we are ignorant alike of the laws of life and the invasions of death. "Let them die, rather than a single animal should be sacrificed." Do these teachers know who it was taught "Ye are of more value than many sparrows"?

IV.

We turn from the fanatical school of men and women who pretend a mercy they fail to practise ~~to~~ those who follow experimentation as a branch of human knowledge. To the majority of these scholars the labour they pass through is infinitely painful, and for our part we know of not one of them who is exempt from this feeling. They are urged by what they consider is the necessity of their research: they are filled with the belief that the work is probationary only: that in time the necessity for experiment will pass away. No one now experimentizes to prove the circulation of the blood, but without experiment such proof could never have been afforded. Meanwhile there are great problems of a similar momentous kind which have to be solved, which bear on life and which can only be revealed from the veritable sight of the phenomena of life.

To attempt to prevent researches of this nature is to our mind as vain as it is hurtful. It is best, we are sure, for the lower animals subjected to experiment to have every experiment performed in a civilised and law-abiding community, where the common-sense and common humanity of the people can curb and keep down all real excess of zeal and all real hardening of heart of which so much has been spoken. To stop experiment in England were but to open it in other places where the very safeguards which here exist against abuse are absent altogether; where no organized and active society exists ready to lay its hand on a positive offender, and where veritable offenders would want the culture and sentiment that belong to the higher order of mind which here prevails. The existence, in a sentence, of a calm and temperate society, that will with the one hand wave back fanatic oppression, and with the other moderate scientific zealotism, is the best condition that can be obtained while experiment is doomed to last. Such a society

truly can no more put down or punish experimentalists in science than it can those sporting and epicurian experimentalists whose labours we have described. It can moderate both.

For ourselves, we are wholly of opinion that experimental physiology should be distinctly separated from those experimental sciences which deal only wth inanimate matter. It is not work that ought to be followed up by the young student, like a course of practical chemistry, or botany or mechanics. The experiments are means to discovery; they are not means to the continued exercise of learning. Least of all are they in any degree training experiments; and though it may be necessary now and then for a master to demonstrate them for the sake of truth, it is the master only that should demonstrate, and even he with all the caution and all the humanity he can reasonably command.

Some zealotism, well meant, but bad in effect and weak in design, has, we believe, raised the present fanatical outcry against all experiment. It has led to the publication of practical physiological text-books for students, to advertisements of experiments designed for students, to a too eager desire on the part of young enthusiasts to advance science by this line of inquiry, and to the too hasty and careless mode of reporting experimental studies. In other departments of research such zealotism passes for nothing; in this it cannot occur without touching somebody in the heart. So soon as the heart is touched down goes the reason, and the primitive injury, however small it may be, is exaggerated, as it is told, until passion distorts every object and lends to the simplest figure the most repulsive form.

V.

It is not to be expected that the abolitionists of animal experimentation for the sake of science will readily slacken their efforts. They will still vivisect for their own pleasures or luxuries, and still cut up the vivisectors as a satisfaction for their own consciences and as a variation and completion of their humane labours. They will continue blind to their own inconsistencies, deaf to the voice of reason that teaches the usefulness of science, and callous to all the claims of humanity save those that knock at their own door. It remains to be seen whether, ignoring these unreasonables on one side and all extreme zealots of physiology on the other, there cannot be found some common ground on which moderate men of both parties may come to a reasonable and clear, though it be but tacit, understanding. It is worthy of notice that men of science have themselves taken the initiative in suggesting a series of rules for the guidance of those who are engaged in physiolo-

gical pursuits. At the meeting of the British Association for the Advancement of Science, held in Liverpool in the year 1871, a committee of the Section of Biology submitted the following rules, which were accepted by the association and published in the 'Transactions.'

"1. No experiment which can be performed under the influence of an anæsthetic ought to be done without it.

"2. No painful experiment is justifiable for the mere purpose of illustrating a law or a fact already demonstrated; in other words, experimentation without the employment of anæsthetics is not a fitting exhibition for teaching purposes.

"3. Whenever, for the investigation of new truth, it is necessary to make a painful experiment, every effort should be made to ensure success, in order that the suffering inflicted may not be wasted. For this reason, no painful experiment ought to be performed by an unskilled person with insufficient instruments and assistance, or in places not suitable to the purpose—that is to say, anywhere except in physiological and pathological laboratories, under proper regulations.

"4. In the scientific preparation for veterinary practice, operations ought not to be performed upon living animals for the mere purpose of obtaining greater operative dexterity."

We have always felt that these rules, good as they are in principle, are wanting in authority. The committee that drew them up was too small and exclusive, and a society which meets but once yearly, and for very varied objects, was hardly fitted for the appointment of a committee for such a purpose. Still, the intention was good, and if a more ancient and authoritative society—the Royal Society, for instance—were to consider and publish a decisive opinion and direction, an important public service would be accomplished.

If no one of the great public scientific bodies undertakes the duty of adjusting the differences between the men of science and their opponents, the matter, as far as we can see, must rest upon the public sense and judgment of what is and what is not right. It is not in the legislative chamber that any common agreement can be found, nor can any Act of Parliament, if it were passed, govern the necessities of science. The agreement must be settled in the community at large. It must rest on the acceptance by the people that a few out of the hecatombs of animals which are slaughtered daily for food or for other necessity of man may be sacrificed instead at the altar of science. It must be sustained by the confidence that those who are the acknowledged professors of science will use the privileges with which they are entrusted in solemn office, dedicated to the good of true humanity and governed by her supreme and rigorous law.

II.—Robin's Lectures on the Humours.[1]

THIS new edition of Professor Robin's lectures (the first was published from shorthand notes in 1867) reminds us forcibly of the growing difference between the physiological teaching of the present generation and that of the generation which is passing away—a difference less of facts than of form and method. The treatise now under review is, perhaps, the most complete one in existence upon the subject with which it deals. The new edition has been carefully revised and brought up to the level of the most recent knowledge we possess, and occasionally embodies the results of inquiries which were actually in progress at the time of its publication; yet its general tone is that of yesterday rather than that of to-day. In what, then, does this difference between the physiological teaching of twenty years ago and that of the most advanced school now among us—which may fairly be represented by the popular text-book of Hermann—mainly consist? Various partial answers may be given to this question. It appears to us, however, that they may be all summed up in the statement that whereas physiology has hitherto been taught—chiefly, if not entirely—in relation to practical medicine, it is now, and will be more and more, taught as a branch of general biology. The fundamental discrepancy between these two points of view is only beginning to make itself felt, but it is pregnant with consequences for the future both of medical education and of scientific progress.

That branch of biology which concerns itself with the dynamics of the living organism was originally a mere offshoot from medicine. The phenomena exhibited by the human body, whether in health or disease, were observed with a view to the discovery of new methods of diagnosis and treatment. The lower animals were called upon to contribute occasionally, and, as it were, incidentally, to the solution of problems primarily affecting the welfare of man; and, naturally enough, those animals were chosen which present the closest analogies of structure to the human species. The number of questions which suggest themselves to the mind of a physiological inquirer being practically infinite, it may readily be understood that when every physiologist was a physician, those questions which had a direct bearing upon medicine were the first to claim his attention. But the time is at hand for physiology to emancipate herself from those trammels which supported her infancy,

[1] *Leçons sur les Humeurs normales et morbides du Corps de l'Homme, professées à la Faculté de Médecine de Paris.* Par CHARLES ROBIN. Second edition. Paris, 1874.

but which are felt to hinder her free growth to the maturity of an independent science. The scientific physiologist of the present day stands in much the same relation to the physician as that which the astronomer occupies towards the navigator. He devotes himself to the study and elucidation of the phenomena presented by living beings, apart from any special reference to the benefit which man may derive from his investigations; one fact to him is as good as another; the vital processes of a sponge-animalcule are as interesting as those of a mammal. This absence of concentration as regards the end in view leads naturally to a corresponding divergence of method. So long as the functions of the human body are the principal object of inquiry, observation must take precedence of experiment. Now, the most striking characteristic of modern physiology is its devotion to experiment. Magendie's vivisections, which excited so marked a feeling of disapproval among the educated classes of Europe in the early part of this century, now form part of the routine work of every physiological laboratory. Magendie's passion for *le fait brut*, his repugnance to anything like theory or generalisation, might seem to have inspired the numerous periodicals devoted to physiological research. The papers they contain are, for the most part, mere laboratory notes, mere descriptions of experiments, which vie with one another in painstaking accuracy, but the deductions from which, even when they seem to flow spontaneously from the facts described, are stated by their authors with almost an affectation of cautious reserve.

What may be the comparative value of these two ways of looking at physiology, when they are weighed against each other absolutely, this is not the place to inquire. It is sufficiently obvious that to the medical reader the older fashion—that of which M. Robin's work is an admirable example—presents many advantages. We have a physician writing for physicians, handling physiology as an instrument for the elucidation of pathological problems, rather than a physiologist addressing a circle of physiologists, and using the phenomena of disease only by way of illustration, or to stop the gaps left by inadequate experimental evidence.

Side by side with the corpuscular elements or nutritive centres of which the tissues of the living body are built up, stand the various humours by which these elements are surrounded, permeated, and nourished. The investigation of the former is the business of histology, while the latter form the domain of hygrology; the two together constituting the subject-matter of General Anatomy. Owing to the great development of microscopic research during the last thirty or forty years, histology has engrossed attention to the comparative exclusion of hygrology,

and it is the main purpose of these lectures to reinstate the latter branch of general anatomy in its due position. The study, both of the solids and the liquids of the animal body, is pursued by means of analysis. The histologist breaks up a compound into its constituents by the scalpel, the needle, the microscope; the hygrologist employs the filter, the test tube, and the reagent for the same purpose. Both are anatomists, though the means adopted by the former to dissociate the tissues are commonly, though erroneously, termed anatomical *κατ᾽ ἐξοχην*, while those employed by the latter are usually regarded as chemical. The difference is a verbal one only, and hardly that; anatomy may be strictly taken to mean a division, a separation—an analysis, in short; and the fundamental similarity of the two departments of study loses none of its force because the means employed are not the same in both.

M. Robin classifies all the liquids of the human body under four principal heads:—*constituent humours,* including blood, lymph and chyle; *recrementitial,* comprising the various serous and synovial fluids, and the glandular products subservient to generation and the nutrition of the embryo; *excremento-recrementitial,* including the varieties of mucus, the lachrymal, salivary, gastric, pancreatic, biliary and sebaceous secretions; *excrementitial,* including sweat, urine, the amniotic and allantoic fluids, and the vapour exhaled from the lungs. A fifth group, somewhat anomalous from a systematic point of view, is formed by the chyme, meconium, and fæces.

Of the thousand pages given up to the description of these various liquids, 300 are devoted to the first three—blood, lymph, and chyle. They are discussed with a fulness commensurate with their importance in the economy; and for this reason, as also because M. Robin's views on some points differ considerably from those generally accepted, we may legitimately consider them in some detail.

In harmony with the general tenor of the work, M. Robin attributes great importance to the plasma of the blood as compared with the corpuscles. We are too much in the habit of regarding the plasma as a mere agent for the distribution of nutrient matter, as a medium in which the red corpuscles, and still more the leucocytes, may display their activity. Since the discovery of the amœboid contractility of the latter elements, and of the part played by their migration in the various forms of inflammation and exudation, we have been still more disposed to ignore the vital endowments of the *liquor sanguinis.* And yet its albuminoid constituents do not yield in functional significance, whether in health or disease, to any of the corpuscular elements in the organism. These "coagulable or non-crystalliz-

able principles of organic origin," chief among which are *plasmine, serine,* and *peptone*, form in the dry state about 78 grammes per 1000 of blood.

Although he gives an exhaustive account of the theory of coagulation known as Schmidt's—that which is commonly taught in this country and in Germany—M. Robin prefers to adhere to the less known views of Denis.[1] Hence his proximate analysis of the plasma differs from that given by Kühne, Hoppe-Seyler, and other authorities. His reasons for preferring the views of Denis are chiefly based on the comparative simplicity of the methods adopted by that inquirer, which reduce the chance of producing artificial compounds in the course of analysis to a minimum. According to Denis, the chief constituent of the *liquor sanguinis* is *plasmine;* this may be procured by allowing the blood to flow from a vein into a saturated solution of sodic sulphate (to prevent coagulation), and then adding powdered sodic chloride. This throws down a white, pulpy precipitate of *plasmine* (25 parts per 1000 of blood), leaving in solution 53 parts per 1000 of *sérine*, a substance presenting many of the characters commonly attributed to the albumen of the blood, but which is not identical with it. When thus precipitated, plasmine is soluble in from ten to twenty times its weight of water; but after an interval of a few minutes it breaks up into a solid clot, made up of *concrete fibrin*, and a liquid, *metalbumin* or *soluble fibrin.* The *peptone* contained in blood-serum is present in very small proportion; it is obtained by coagulating the serum with hot acetic acid, and throwing it on a filter; the liquid which runs through contains from ·001 to ·004 of an albuminoid substance, the *albuminose* of Mialhe or peptone.

The molecular mobility of these coagulable principles of the plasma constitutes its life. "The *plasma sanguinis* is endowed with life; it assimilates nutrient matter; it undergoes simultaneous decomposition and recomposition without being destroyed; and these changes take place with rapidity and energy.

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[1] An excellent summary of the experimental evidence on which the commonly received theory of coagulation is based, may be found in Sanderson's 'Handbook for the Physiological Laboratory,' p. 166. M. Robin gives an account of the more recent researches of Alex. Schmidt (1872), from which it would appear that a third factor is necessary for coagulation, a ferment without which the characteristic reaction between fibrinogen and fibrinoplastic matter cannot occur. So long as the latter substances are absolutely pure no coagulation takes place, and the rapidity with which fibrin is produced by their union depends on the amount of ferment present. The ferment itself undergoes no change, and may be separated from the clot by filtration. M. Robin argues, with much show of justice, that this theory complicates matters more than it explains them, and regards the ferment isolated by Schmidt as in all likelihood due to commencing decomposition of an albuminoid material.

The life of the plasma is a coequal factor with that of the corpuscles in the life of the blood."

It is in its application to the vexed question of contagion and its material basis that the interest of M. Robin's views concerning the molecular vitality of the "constituent" humours naturally culminates. The characteristic of any infecting substance—that which distinguishes it from all the varieties of poison, whether of animal, vegetable, or mineral origin—is its power of indefinite multiplication in the infected organism. This it is which obliges us to regard it as endowed with vitality, whatever be the meaning we attach to the term. So far most pathologists are agreed; beyond this point all is doubtful. We may assume that contagion is propagated by particles of bioplasm derived from the affected organism, whose living tissue-elements have undergone some ill-understood vital change or degradation, which unfits them for their normal functions, and gives them a fatal power of multiplying in any healthy organism into which they may penetrate. We may regard all infectious diseases as a result of the introduction of parasitic organisms, probably of vegetable nature, into the infected body, where by their independent life and growth they occasion, directly or indirectly, the chain of phenomena constituting the entirety of a specific disease.

M. Robin's theory is distinct from either of the above, though in one way it may be taken as a humoralistic counterpart of the former view. The essential feature of all zymotic diseases (the term "zymotic," be it said in passing, is one which M. Robin very justly repudiates) is a profound molecular alteration of the plasma, and, in particular, of its coagulable principles. This alteration must in no wise be confounded with, or even assimilated to, a fermentation. A true fermentation involves the dissociation of a compound into two or more simpler substances—(*e. g.* of glucose into alcohol and carbonic acid, of amygdalin into benzoyl hydride, hydrocyanic acid, and glucose). No such dissociation of any constituent principle of the blood takes place; the change is rather one of isomeric metamorphosis, a change of molecular arrangement without any alteration of centesimal composition, analogous to that which occurs in the conversion of inflammable into amorphous phosphorus, of starch into dextrine, &c.

This is the primary and fundamental process. The intimate nutritive relation between the plasma and the formed elements, whether stationary (tissue-elements) or migratory (leucocytes, &c.), necessarily involves the contamination of the latter; they are secondarily affected by the morbid change, and acquire the same specific properties. The profound disturbances of nutrition caused by this modification of all the liquids and solids of the

body are amply sufficient to explain the various functional troubles which invariably follow infection.

So far, though we are treading on very uncertain ground, M. Robin's views are not open to *à priori* objection. But how does he explain the main feature of contagion? How comes it that an infinitesimally minute particle, derived from the diseased organism and transferred to one which is still healthy, is capable of converting the latter into a pathological *replica* of the former one? If we suppose the infecting particle to have the power of reproducing its kind in a suitable medium, we overcome this difficulty at once. But M. Robin meets it in another way. He asserts that the smallest quantity of matter from the diseased organism, containing the coagulable albuminoids in a state of molecular change, is capable of inducing a gradual change of exactly the same kind in the coagulable principles, first of the blood, then of the tissues, of a healthy individual. The change is wrought by an ever-widening process of catalysis, each particle communicating its own molecular state to those next it, and so on till all the liquids and solids capable of undergoing the change in question are alike affected. The interval during which this infection by contiguity is taking place forms the incubative stage of the disease.

We may fairly ask what evidence there is of such molecular modification of the coagulable principles of the humours, and whether that evidence be sufficiently cogent to induce us to set aside the ever-increasing weight of testimony in favour of the parasitic theory of contagion? Prof. Robin illustrates his theory by applying it to the pathology of cholera, and thus furnishes an instance by which its power to explain all the phenomena of a general disease may be tested. Further, he enters on an elaborate criticism of the facts on which the parasitic theory appears to him to rest.

Let us consider, first, his interpretation of the symptoms and structural changes associated with cholera. The coagulable principles of the blood and lymph are characterised by their power to take up and hold in combination a large proportionate amount of water, which they may be made to give up without undergoing decomposition. The plasma is constantly taking up water holding various mineral and organic salts in solution, and constantly yielding up water to the tissues and secreting organs, and this without its albuminoid constituents undergoing any profound chemical change.

The first step in cholera is the entrance of a miasm into the healthy organism through the medium of the inspired air. This miasm is an albuminoid substance in a peculiar state of molecular change. It enters the blood, where it gradually commu-

nicates its own molecular condition to the albuminoid elements of the plasma. The chief effect thus wrought in them is a loss of their power to hold water in combination—to fix it; their dehydration entails an abundant flux of the superfluous and uncombined water, holding salts, &c. in solution, from the capillaries of the alimentary mucous membrane. The choice of these capillaries is determined by their comparative nearness to a free surface—is therefore purely mechanical. The exosmosis of liquid into the bowel entails a corresponding cessation of absorption from its mucous surface. The volume of the plasma being enormously reduced by loss of water, the pulse grows thready, the patient is tormented by thirst, and the nutritive interchange between the blood on the one hand and the tissue-elements and secreting cells on the other is arrested. Hence the muscular cramps, the diminution and ultimate suppression of the biliary and renal secretions, &c. Further, the blood-corpuscles preponderate unduly in the lessened volume of plasma. They tend to stick together; they are arrested and accumulate in the capillaries, where they part with their oxygen and become loaded with carbonic acid; unable to return to the air-cells of the lung for renewal, the deoxygenated blood assumes a violet hue; hence cyanosis of the skin and tissues. The exosmotic current from the capillary network of the intestinal mucous membrane softens and strips off its epithelial coat, leaving the sub-epithelial surface naked and without protection.

It is hardly necessary to enter into a detailed criticism of this hypothesis concerning cholera. If a legitimate hypothesis be one which affords an adequate explanation of *all* the phenomena observed, whether the explanation be correct or incorrect, it is clear that the one whose main features have just been sketched has little claim to be accepted, even provisionally. But though we are constrained to admit the insufficiency of M. Robin's constructive effort, it must be allowed that his critical examination of the views held by the advocates of the "parasitic germ-theory" is infinitely stronger. His main objection is founded on the morphological identity of the organisms present in the blood of the most different forms of infectious disease, and in the mouth and alimentary canal of healthy individuals. How can it be asserted, he argues, that organisms are the cause of such diseases, when these organisms are undistinguishable, passing, according as they are placed under particular conditions, from the state of microzymes to that of bacteria, and from this again to the jointed filaments of *Leptothrix*. Those who so far agree with Hallier's views as to regard the microzymes as similar rudimentary stages in the development of certain higher forms, morphologically distinct from one another, have never

been able to establish their position on a sound experimental basis. Hallier's own attempts at cultivation, as is now generally recognised, ended in a maze of confusion.

This argument is undoubtedly a powerful one. It leaves too much out of sight, however, that morphological similarity—especially in the case of organisms so minute as to be visible only under the highest powers of the microscope—need not necessarily mean morphological identity; and further, that functional diversity may possibly be consistent with morphological identity, or what to our weak means of observation may appear identity. The ova of various mammals, for instance, might reasonably be regarded as identical by observers unacquainted with the potential differences they embody.

The case of hereditary syphilis is at first sight perplexing. When an embryo receives its taint from the male parent, without intermediate contamination of the female, are we to assume that specific organisms were conveyed by the spermatozoa to the ovum? There are two possible explanations. One is that the maternal blood is always primarily infected, although the taint may not manifest itself by any symptom; the other rests on analogies derived from the singular observations of Pasteur on the hereditary transmission of *pébrine* among silk-worms.

Finally, M. Robin lays great stress on the experimental results obtained by Onimus, who believes himself to have proved that bacteria, when separated from the septic or virulent liquids in which they float, cease to exhibit any infective power. The experiments were performed by placing blood taken from pigs suffering from typhoid fever in a dialyzer, and floating this in distilled water at a temperature of 35° C. After the lapse of about fourteen hours the water is seen to be opalescent, and is found to contain a multitude of organisms identical in form with those contained in the blood. Two series of rabbits are then inoculated; one with the blood contained in the dialyzer, the other with the water swarming with microzymes. The first set quickly die of septicæmia, the second set survive. Hence M. Onimus concludes that the microzymes are not the determining cause of infection, but that the *materies morbi* consists of some albuminoid substance in the blood which is incapable of traversing a colloid septum.

Granting that the experiments are correct, does the conclusion inevitably flow from them? May we not fairly assume that the organisms in the water are putrefactive microzymes rapidly developed at the expense of the saline and other crystalloid constituents that have filtered through—organisms specifically distinct, not even remotely descended from those on the other side of the septum, and hence endowed with none of

their pathogenic properties? Distilled water, as we know from the researches of Sanderson, invariably contains the germs of bacteria, which are developed as soon as pabulum for their nutrition is supplied. Again, the experiments of Chauveau and Sanderson showing the particulate nature of the contagium of small-pox, vaccinia, and glanders, prove that the liquid into which the albuminoid as well as the saline ingredients of the infective liquid have passed by diffusion is incapable of communicating disease. In the face of such testimony the experiments of Onimus are far from decisive.

M. Robin is willing to admit that bacteria may serve as carriers of contagion, but only in the sense in which *any* solid particle, impregnated with the virulent liquid in which it happens to float, may act as a carrier of contagion. He regards them as epiphenomena of disease, developed and multiplying in humours which have already undergone a beginning of molecular change in consequence of their contamination by a specific albuminoid virus. Although the question must still remain open, —although the confusion into which Hallier and his disciples have been led, the negative results of Lewis and Cunningham's researches on cholera, and many other circumstances, may seem to have rendered the parasitic theory of contagion highly problematical—there is, perhaps, more to be hoped for in this direction than in any other. No student of the subject should neglect to read M. Robin's chapter upon it, and he will probably rise from its perusal with the feeling that, although the difficulties in the way of accepting the parasitic hypothesis of contagion are many and great, there is no other view with more, if so much, of positive evidence to back it.

An elaborate description of pus, its mode of formation, its varieties, and its occasional virulence, is logically connected with that part of the chapter on the blood which we have just been considering. The author defines pus as "une humeur de production accidentelle, hétérotopique, mais non hétéromorphe, liquide ou demi-solide, variant du grisâtre séreux au jaunâtre crémeux." The principles of which it consists are derived from the blood; but the chemical composition of the *liquor puris*, though it may vary within tolerably wide limits, is always different from that of the plasma, and cannot be regarded as a simple exudation or transudation from the vessels. The special properties of pus, whether physiological or pathological, belong entirely to its liquid part, the leucocytes which float in it being relatively unimportant. The *liquor puris* differs in composition and in its dynamic properties with the locality in which suppuration has taken place, and with the condition of the organism at the time; the leucocytes, on the other hand, are structurally and functionally identi-

cal with one another, whenever and wherever they may be generated; their proportionate quantity may differ, but not their quality. They originate in the blastema, and are not derived by proliferation from the corpuscular elements of the tissues, or by migration from the blood in the capillaries of the inflamed part. They determine the greater or less degree of turbidity of the pus, but contribute nothing to its specific properties. Here, as in the blood, we must look to the albuminoid elements of the liquid in which the corpuscles are suspended for what physiological energy the humour may possess.

Such thoroughgoing humoralism as this can rarely be met with in the pathological literature of the present day. So much assiduity has been devoted to the study of the corpuscular elements of pus, their origin and destination, that the liquid in which they float has, perhaps, been unduly neglected. Two points in the history of the pus-corpuscle may surely be regarded as certain; one, that it may result from the division and multiplication of any living tissue-element; the other, that it may be derived from the blood, from whose colourless corpuscles it is undistinguishable, by a process of emigration or diapedesis. It is hard to see why M. Robin should have set aside the mass of converging testimony on which these notions are founded, unless we admit the possibility of his having been influenced by an unconscious desire for systematic consistency, even at the expense of opposition to established facts.

To be hurtful to the organism in which it is produced, or into which it may be inoculated, pus must either be fetid or virulent. Great stress is justly laid on the difference between these two conditions, the former of which may be remedied with comparative ease, while the latter is probably irremediable.

The fetidity of pus may be primary or derived. If primary, it is a result of the putrefactive decomposition of its albuminoid constituents, attended by the evolution of mephitic volatile products, such as sulphuretted hydrogen, hydro-sulphate of ammonia, traces of phosphoretted hydrogen, &c. This decomposition is intimately connected with the access of air; hence even abscesses devoid of any external opening, when situated near exposed mucous surfaces, as, *e. g.*, in the neighbourhood of the mouth, pharynx, and trachea, may contain extremely putrid pus, a fact long known to surgeons. (To those who adopt the germ-theory of putrefaction such facts are susceptible of being otherwise explained; pus contained in an abscess near the pharynx is apt to putrefy, not because oxygen may filter through its walls, but because the septic organisms normally present on the free surface of the mucous membrane may obtain access to its interior.) Or pus may derive its fetor from

odorous substances in its neighbourhood which traverse the intervening layer of tissue by osmosis; thus the contents of an abscess may have a urinous, fæcal, or other odour.

Fetid pus, should its escape be hindered, may give rise to symptoms not unfrequently confounded with those of septicæmia. Such symptoms are really due to the gradual absorption of poisonous gases into the circulating fluid. Exactly similar phenomena may be produced by the injection of the volatile products of putrefaction into the systemic veins.

The reason why such absorption does not give rise to the fatal accidents which might be anticipated from the intensely poisonous nature of the gases in question has been pointed out by Bernard. Poisonous effects are produced only when the toxic agent is brought into direct contact with the tissue-elements by the arterial blood; now, when a very volatile gas, such as sulphuretted hydrogen, is introduced into the systemic veins, whether by injection or osmosis, it will never enter the arterial system so long as the rate of its introduction does not exceed that of its elimination by the lungs. It is only when the gas enters the blood faster than it can be got rid of in the expired air that poisonous symptoms ensue, proportional to the amount of gas which penetrates into the left heart. The symptoms thus produced are those of "putrid intoxication," and must be carefully distinguished from those of septicæmia in all its forms. The latter can only be caused by pus which is virulent, and the virulence of pus is owing to a molecular change in the coagulable principles of its liquid part, precisely similar to that which occurs simultaneously in the coagulable principles of the blood and lymph. The virulence of pus is independent of whatever formed elements may be suspended in it, whether leucocytes or bacteroid organisms.

The chapters on the urine form, as might have been expected, a tolerably complete treatise on the chemistry of that secretion both in health and disease. They are followed by an excellent account of the structure and composition of urinary calculi. The subject has been so thoroughly investigated during the last twenty or thirty years that novelty can hardly be looked for in its treatment. On one point, however, viz., the acidity of normal urine, M. Robin adopts the conclusions of Byasson, which differ somewhat from the views contained in our text-books. Most authors attribute the acidity of the *urina sanguinis* to the presence of the acid phosphate of soda, together with small quantities of free organic acids, such as lactic, oxalic, acetic, &c. Liebig's fundamental experiment, on which this view is based, consists in heating a solution of the neutral phosphate with uric acid (which does not redden litmus); after

the liquid has been allowed to cool it is found to present an acid reaction, while a deposit of crystals of urate of soda leads to the conclusion that an equivalent of base has been abstracted from the neutral phosphate. This experiment seems to have yielded negative results in Byasson's hands. He finds that uric acid exists in normal urine under two forms, independently of urates, viz. in the free state, and conjugated with neutral phosphate of soda to form what he terms a uro-sodic phosphate, a sparingly soluble compound, more soluble in urine than in pure water, though imparting an acid reaction even to the latter fluid. The acidity of fresh urine is partly due to this compound, and partly to the presence of free carbonic and hippuric acid. An artificial urine, prepared by dissolving urea, uric acid, hippuric acid, carbonic acid, phosphate and chloride of sodium, and sulphate of magnesia, in their due average proportions, in distilled water at a temperature of 40° C., forms a clear liquid with a decidedly acid reaction, which deposits a few crystals of uric acid after the lapse of forty-eight hours.

It is a well-known fact that healthy urine, guarded against accidental contamination, increases in acidity for a variable number of hours or days after it is passed. This curious phenomenon was attributed by Scherer to the production of free lactic acid (and often acetic acid, as pointed out by Lehmann) by the operation of a ferment (the vesical mucus) on the urinary pigment. Byasson denies the existence of these acids in a free state in healthy urine, and has recourse to a somewhat obscure hypothesis to explain its increased acidity. He suggests that several of the urinary constituents, owing to the mode of their secretion in the kidney, are so conjugated that the distinctive properties of each are partially masked. Their mutual affinity being feeble, it is gradually relaxed after the discharge of the urine from the bladder, and changes in its reaction ensue in consequence.

In harmony with his general tendencies M. Robin is inclined to ascribe a far smaller share to living organisms in the causation of ammoniacal urine than is usually claimed for them. He discusses the question whether the urine ever becomes ammoniacal in the bladder without the previous introduction of germs by the sound or catheter, and he answers it in the affirmative. A similar answer was given by Ricord, Gosselin, and other surgeons, when the matter was lately brought before the Academy of Medicine. Pasteur's assertion that the urine never becomes ammoniacal without the previous entrance of germs into it from without may still hold good, however; it would be necessary to prove that such germs cannot reach it except through the urethra, and this is by no means certain.

In the section on chylous urine an account is given of the discoveries of Dr. Lewis, of Calcutta, with a figure of the nematoid hæmatozoon to which the phenomenon is apparently due. The author disapproves altogether of the term "chyluria," implying as it does a belief that the abnormal constituents of the secretion are derived from the lymphatics of the urinary tract. He prefers the old name of "hématurie graisseuse" given by Rayer. The oily matters in the urine come directly from the blood, whose serum is persistently maintained in a milky condition by the operation of the *causa morbi;* moreover, red blood-corpuscles are almost always present, occasionally appearing in such numbers as to convert the so-called "chyluria" into an indisputable "hæmaturia."

In bringing this notice of M. Robin's lectures to a conclusion, it is fair to say that out of the multitude of subjects with which they deal we have chosen a few only for discussion, either because the author's statements appeared open to question or because they happen to be connected with the problems of the hour. To give anything but a one-sided account of what is really a huge work of reference, comprising a thousand closely printed pages in large octavo, and ranging over a great part of physiology and pathology, would obviously be impracticable, and probably undesirable, in a review. But though so wide in their scope, there is hardly a subject in these lectures to which the author has not contributed some results of original work; and though exhibiting a very extensive acquaintance with foreign as well as French biological literature, they have none of the characters of a mere compilation; the individuality of the author is felt throughout, and the various matters discussed are held together by an elaborate systematic framework, and have a clue of logical consistency running through them. So that, making allowance for the slightly dogmatic tone adopted in speaking of some questions which can hardly be considered settled, a tone not, perhaps, ungraceful in a veteran who has done so much to enrich and to systematise our knowledge, we cannot part from them without stating that they form a most valuable addition to the library of every advanced student, and a worthy monument of the laborious industry and research of their author.

III.—Reports on Leprosy.[1]

There are various acute febrile affections, the dread of catching which, has led to the temporary seclusion of those who suffer from them. But there is only one chronic purely physical disease, which from the earliest ages has inspired so much horror, that those affected with it have, in the great majority of countries, been shunned by their neighbours. To many of the busy practitioners of Europe probably the disease is indeed merely a name. It has died out in Great Britain and in most parts of Europe where it once prevailed; yet it is still one of the most widely spread maladies that afflict humanity. It is probably the oldest constitutional affection of which we have any account, and there is no reason to suppose that the disease has varied or modified itself in the slightest degree, even in the smallest particular, in the course of ages.

The subject of leprosy must, we fear, continue to be for many a day to come, one which will deserve the serious attention of our Colonial and Indian Governments, for the disease is widely spread through our great Indian possessions, is common in some of our West Indian islands, is seen at the Cape and in the Mauritius, and is not entirely unknown in our North American colonies. Medical treatment has always been able to do something, but it has never done a great deal, for leprosy; it certainly has not contributed much to its extinction; and it has been usual to ascribe the decline of the disease in this and other countries, mainly to improved hygiene and improved conditions of life, which have been aided by segregation, whether in the form of leper hospitals or villages, or by the, as it were, instinctive feeling of mankind, which has usually led to intercourse with lepers being avoided.

At the present time it appears to us, that there is a tendency to revive rather exaggerated notions of contagion, such as were once prevalent, and also to attribute to hereditary transmission too exclusive a share in the propagation of the disease. Upon these two principles Government has been urged, not only to the revival and improvement of leper hospitals, but to the enforcement of an amount of compulsory segregation, such as has scarcely ever practically existed.

As, notwithstanding that the profession has long had before

[1] 1. *Report on Leprosy and Yaws in the West Indies.* By Gavin Milroy, M.D. London, 1873.

2. *Report on Leprosy and Leper Asylums in Norway with reference to India.* By H. V. Carter, M.D. London, 1874.

3. *Handbuch der Speciellen Pathologie und Therapie der Hautkrankheiten.* III vol., article "Lepra." By Dr. M. Kaposi. Erlangen, 1872.

it the large amount of useful information collected and arranged by the College of Physicians in their Report of 1867, a good deal of misunderstanding appears to prevail on the subject, a review of the question in its more general relations, may help to enable us to form sound opinions. The old *historical* aspects of the subject are less known than they deserve to be, and have a direct bearing on the questions under discussion, and, indeed, in a practical point of view, are just as valuable as the more recent ones. We shall therefore give a summary of them.

The labours of Hensler, including the history of leprosy in all ages, the learned studies of Sir James Simpson and of Virchow on the disease in the middle ages, afford ample material for such a survey. Dr. Kaposi's clear outline has been a valuable guide to us, and the original authors have not been neglected.

It is unnecessary to recite in detail the symptoms of true leprosy,—the elephantiasis of the Greeks, which are the same now, as when they were described 2000 years ago, nor need we enter into the question of the particular form of skin disease, which passes in the Bible under the name of leprosy. It was not elephantiasis. Nevertheless, there is little doubt that elephantiasis was common at a very early period in Egypt and in Syria, as it is now in Palestine. Hippocrates calls it the Phœnician disease. About the time of Augustus, and shortly after it, Celsus and Aretæus give between them, a tolerably minute and quite characteristic account of the malady, and Aretæus and Cœlius enter largely into its treatment.

With respect to its nature most thought it to be a general corruption of the blood; that it first attacked the internal viscera, and afterwards showed itself on the surface. Aretæus believed that there were granules diffused through all the organs, after the fashion of measly pork, a statement lately repeated almost in the same words by Dr. Mongeri, of Crete, probably in ignorance of Aretæus! but some thought elephantiasis an affection only of the skin. The disease was considered to be highly contagious; it might be acquired by merely eating with lepers. The use of impure water was frequently assigned as a cause of it. Galen attributed it to the moist climate of Alexandria, and especially to the food common there, pulses, and particularly salt fish and shellfish. He said that the Scythians did not suffer from it, as they lived on milk. The early symptoms of the malady were usually overlooked, and the disease was fully established before it came under treatment. In the earlier stage, bloodletting and a great variety of local measures, were employed; among modes of treatment, keeping the skin soft and moist with oily applications was, perhaps, the most common, along with suitable exercise, particularly swimming; change of

diet, and the use of clean clothing, were enjoined. Cœlius thought that he could cure patches of leprosy in its early stage, and that the patient, if not entirely cured, might at least reach the end of his days, without being deformed by the disease. If the disease was once established, all writers recommended change of air, residence in inland places, voyages, and mineral waters, particularly warm sulphur ones. Segregation of lepers was frequently practised. Lepers retired to the mountains to live as hermits, sometimes supplied with food by their relations, sometimes left to starve or to commit suicide, which they occasionally do at the present day in India and in China. Cœlius says that some recommended that strangers who suffered from the disease, should be confined in a city where the malady was unknown, but that citizens afflicted should be sent to cold and inland places, and be recalled when they were cured, as there would no longer be risk of contagion; but he considered that this was rather to exile than to cure a patient, and he doubted whether it was consistent with the humanity of medical practice.

As to the diffusion of the disease, Celsus says it was not known in his day in Italy, but was common elsewhere. Aretæus mentions that Celtic or Gallic balls of soap were a good remedy, from which it has been inferred that it was known in Gaul. Galen represents the disease to be comparatively rare in Germany and in Assyria. Pliny says that leprosy had been brought into Italy in the days of Pompey the Great, but that it had disappeared by his time. Nevertheless, from the accounts of medical writers, it is evident that the disease was not infrequent in Italy during the three or four centuries after him. Sanscrit accounts about this period, according to H. H. Wilson, show that the disease was common in India. The symptoms are fully described. Anæsthesia is prominently mentioned, so also pricking and burning of the feet, which may be analogous to hyperæsthesia. The treatment was chiefly by oils, used externally and also internally. The strongest belief in its contagiousness was manifested by legislative enactments against lepers. About the year A.D. 500 the disease was evidently very common in Italy, and in the time of Heraclius, a pope signalised himself by curing a leper by his holy kiss, a procedure in which he was in later periods followed by various royal personages, especially by ladies.

The Lombards appear to have been specially tainted with the disease, and Rotharis as early as 641 enacted stringent laws against lepers, pronouncing civil death against them, and laws of like severity were re-enacted by Pepin and Charlemagne. Virchow's researches show that leper hospitals had been estab-

lished in France in the 7th century, or 400 years before the first Crusade, and there is evidence of their existence in Germany and in England in the 8th and 9th centuries. The Anglo-Saxons knew elephantiasis, and had their remedies for it. Baths, too, were set aside for lepers; those of St. Trond in 1060, and those in Bath probably at quite as early a date. When lepers were consigned to asylums in those early periods the clergy performed a sort of funeral service over them, as being cut off from the world. They were dressed in serge, were obliged when they went out, to ring a bell or sound a wooden clapper or rattle, to warn every one of their approach. They received alms in small boxes attached to long sticks. Leper or lazar houses have given their name to modern lazarettos, and many of their strange regulations to be met with to this day in Italy, have been handed down from the old leper houses. But if lepers were shut up without sufficient cause, they had a power of appeal for their release to the king. Lepers were then, as in India at the present day, regarded as specially stricken of God, and they often met with much attention. Indeed, in some places lepers were allowed to hold high office in the army and in the state. Many of the knights of St. Lazarus, a body devoted to the cure of lepers, themselves suffered from the disease, and their grand master was usually a leper. The garb of a leper was the best passport for the receipt of charity, and the town of Haarlem had the right of allowing its beggars to wear the leper dress, in order to be the more secure of obtaining alms.

We thus find that in some places there was strict seclusion, in others free communication with the world.

Relaxation of strictness of rules in the west certainly dates from the end of the 12th century, and there is no evidence that it interfered in any degree with the gradual disappearance of the disease, which commenced about fifty years afterwards.

The leper hospitals were carefully organized, and had chapels attached to them. The inferior clergy were sometimes unwilling to give their services in the chapels, and they occasionally, as in 1177, exacted tithes from leper hospitals, whose inmates tilled the fields, which shows that they were made to employ themselves. The pope, when appealed to, usually decided in favour of the lepers. The canonical law (1237), with its view of the sacrament of marriage, did not sanction the separation of man and wife on the ground of leprosy. Indeed, it expressly

[1] Commentators in later years raised the question, whether syphilis or phthisis, or other contagious diseases, were not equally good grounds for separation; and as to children, they doubted whether it was not better that they should be lepers, than not be born at all.

enjoined cohabitation, in spite of the risk of there being offspring, unless the disease was very far advanced in one of the parties. However, if leprosy came on after betrothal, the marriage need not be proceeded with.[1] Still, lepers, if not married, were to live apart from others, and a leper priest was not allowed to celebrate mass.

Although leprosy was thus widely diffused in Europe before the Crusades, nevertheless, the return of the Crusaders, in however diminished numbers, may have favoured the spread of the disease in the commencement of the 12th century. The Crusaders suffered many privations and often remained quite long enough in the east for them to acquire leprosy, but it seems doubtful whether they could have added much to the mass of a disease already widely prevalent.

A good idea of the disease, as it prevailed in the west of Europe, may be got from an account which has been recently brought to light (by Canon Robertson, of Canterbury, who has communicated it to us) of the miraculous cures of certain lepers at the shrine of St. Thomas of Canterbury, probably about 1180. The patients are described as having tubercles on their faces, running of the eyes, disappearance of the eyebrows, knotting and contraction of the hands and fingers, ulcers of the extremities reaching the bones, hoarse voice, &c. Among them were men and women, also two boys about the age of puberty. By nation they were English, Irish and French. In two instances brothers of patients were similarly affected. One case is narrated, where the disease is stated to have been acquired immediately after sexual intercourse with a prostitute. Many of the lepers had visited various shrines, before they were cured at Canterbury. One had been to Jerusalem and bathed in the Jordan, where there was an establishment for lepers. There appear to have been plenty of leper houses under regular management, in which some of them had been, and where they had worked more or less for their living. In the case of those who were cured at the shrine, they were first examined by doctors, and, if found to be cured, they were restored to society. One patient was partially cured, and the cure completed by ordinary doctors on his return from the shrine.

The Arab writers, the series of whom concludes about this period, add very little to our knowledge of the disease. They seem to have divided it fancifully into four forms—Leonina, Elephantina, Alopecia and Tyria—in accordance with various supposed states of the liver and the bile, and analogies with animals.

Leprosy was particularly prevalent in the end of the 12th and beginning of the 13th centuries; the number of leper houses in France reached its maximum about the year 1244. Vapour

baths and piscinæ were set apart for them at mineral water stations. But common though the belief in contagion was, there was not absolute consent on the subject. Lepers, and patients with ordinary "scabs and tetters" bathed together in the Cross bath at Bath. The baths at Acqui had been always common to all patients, but it was resolved to exclude the lepers. Suddenly the supply of water ceased miraculously, and did not return, until the bath was again thrown open to all!

Leprosy and all matters connected with it, were fully described by the writers who preceded the revival of literature. The monk Theodoric pointed out, besides the early appearance of maculæ and of knots on the skin, the insensibility of various spots on the extremities, a fact of which Bernard Gordon and all the writers of that age were fully aware. They all had a strong belief in the contagiousness of the disease, and believed it to be much more common in men than in women. Gordon treats of leprosy next after scrofula. He considered it to be akin to cancer, to be a disease of the whole system, of which the affections of the skin and of the extremities and face were only the local manifestations. They all believed after Aretæus, in local deposits of leprous matter in the viscera, and especially about the root of the tongue and the larynx. The blood was supposed to be viscous, and to contain sandy particles; the urine was believed to be loaded and very characteristic. The two last notions were certainly very prevalent up to the commencement of the 18th century. Among the early writers, the surgeon, Guy of Chauliac is particularly clear and distinct. He says there are two things—the leprous disposition or habit, and the actual disease. He remarks that the causes of the disease are chiefly these—corruption of the air, bad food, contagion, inheritance. He gives directions as to the mode of examination to be pursued in ascertaining the presence of leprosy, many of them childish to us; such as observing whether blood came when needles were run into the anæsthetic spots, whether a few grains of salt thrown on the blood dissolved or not, whether powdered salt of lead sunk in the urine or not. Many of these inquiries were evidently carried on by the clergy, but physicians had their share also. There was always the difficulty to determine, how far it was sufficient in the early stages only to warn patients to keep at home, and avoid their neighbours. Relations also sometimes advanced false charges of leprosy, to obtain succession to property. The legal segregation of a leper was so serious a step, that it should never be enforced, unless signs of the disease showed themselves in the face.

It seems probable that from the first, no very great care was taken in the selection of lepers, and that leper houses were often

a refuge for the poor and the dissipated, any skin affection being allowed to pass for leprosy. In the commencement of the 17th century, when the disease had greatly diminished, and the King of France ordered an inquiry into the state of leper asylums, it was found that they contained scarcely any lepers. Besides regular leper houses, there seems all along to have been villages of lepers, in which leper men and women and their families lived, some of which survived for a long time in Europe. Such are, indeed, to be seen at this day in the Levant, in Burmah, in China, and doubtless elsewhere. Where also there were only single or few lepers, they were, as is often the case at the present day in the east, simply made to live outside the gates of cities in huts or sheds.

There is little more to be said about the disease at this period. We constantly hear of its being healed chiefly by local treatment, often at the hands of noble or religious ladies or priests, who appear never to have contracted the disease from dressing the sores. Patients who could afford it, visited the various shrines, or the schools of Montpellier and Salerno in hopes of cure. In the 14th and 15th centuries the disease gradually decreased, for instance, in the year 1350 there were scarcely any lepers to be found in the hospital at St. Alban's. The disease did not die out at the same rate in all countries. Thus, it lingered much longer in Scotland, than in England. The disease continued to diminish, and had become comparatively rare in Europe, when syphilis broke out in 1494.[1] A considerable number of the early writers on syphilis were inclined to regard it as a new form of leprosy, but after a time the essential difference of the two diseases was recognised by the great majority of them. Two of the chief arguments for their difference were, first, that syphilis had a special connection in its origin with the genital organs, which leprosy had not; and secondly, that mercury could cure the latter, but not the former disease. We need not trace the gradual decrease of leprosy in Europe, till it is only to be found on the coast of Norway, in parts of Spain and Portugal, some spots on the shores of Italy, and the Greek islands; but a few words may be said on the views of medical writers at a later period.

The diagnosis of the disease seems always to have been more accurate than is commonly supposed. There was indeed over-refinement in the distinction into four forms by the Arabs, which was taken up by early European writers. Lommius (1560) said the disease was contracted spontaneously, by contagion, and by hereditary transmission. In 1652 it appears from Johnston's 'Idea

[1] Curiously enough a monkish author—Raphael of Volterra—writing in Italy in 1469, says, "that leprosy was again becoming more common;" probably he confounded leprosy and syphilis. The French are still apt to do so.

Medicinæ' that every one was aware that true elephantiasis was different from the leprosy of the Hebrews, as well as from the elephantiasis of the Arabs. Scurvy about this period acquired increased extension, although we know of no change of diet or of other hygienic circumstances, to explain its spread. Various analogies between scorbutus and leprosy were observed. Patches of discoloration, hard knots, heaviness of the extremities, ulceration, fetid breath, were common to both; so also dulness, hebetude, and insensibility of the patients. Scorbutus was in those days regarded both as extremely contagious and as hereditary. Causes which are believed to produce leprosy, such as the use of salt meat and of smoked and dried fish, were considered to be the causes of scorbutus. Therefore it is not very surprising, that a visitor to England at this period was struck with the resemblance of cases of scurvy to leprosy, and that Ethmüller regards leprosy as an "exalted scurvy." In the course of the 18th century little new light was thrown on the disease. Some of the earlier symptoms of it, as the formation of phlyctenæ resembling the effects of burns, are mentioned by Schilling in his account of the disease in Surinam in 1770. Robinson, in 1818, pointed out more markedly the distinction of the forms of leprosy, into anæsthetic and non-anæsthetic, a division which has been kept up by most writers, whether as maculate and tuberculose, or, according to the College of Physicians, tuberculate and non-tuberculate.

The great service rendered by Danielssen and Boeck, about 1846, was their proving the identity of the Norwegian spedalkshed with the leprosy of the rest of the world, and their discovery of alterations in the nerves and sheaths, which explained the anæsthesia, not that similar changes may not take place in other affections. They were followed by Virchow and Carter in the examination of the nerves. A great deal of general information respecting the disease was brought together by the College of Physicians in their valuable report. Dr. Milroy has since then contributed an account of the present state of leprosy in the West Indies, as the result of his mission by the Colonial Office, and Dr. Carter gives us the latest news of the experiment in leper houses, that has been made in Norway. Many local Indian reports afford additional light. From all these sources we shall endeavour to glean a brief outline of the present state of our knowledge respecting the *pathology* of the disease, chiefly of the additions which have been made to what has always been known.

As to the early symptoms, the Norwegian observers appear to have made out in some cases, a prodromal stage of catarrho-febrile disturbance. This has scarcely been confirmed from

other quarters, where only a feeling of lassitude and of coldness and general malaise has been observed. In Norway hyperæsthesia is said to precede anæsthesia in many cases. This, too, is scarcely confirmed elsewhere, unless the symptom is represented by the pricking and burning in the extremities which is often complained of. "The structural changes observed," to use the words of Dr. Carter, "are due to exudation or deposit in the skin and nerve trunks of a firm, translucent, colourless, or pale reddish material, which may be distinguished by the terms hyaline-fibroid and hyaline-granular. As regards the skin, conjunctiva, or mucous membrane of the mouth, the hyaline-granular deposit first appears within or immediately beneath the cutis or mucous membrane. In the nerves, the hyaline-fibroid deposit first appears between the individual nerve-tubules and within their common envelope; the outer sheath of connective tissue is hardly changed. By accumulation of the new material the nerve-tubes are separated, compressed, emptied, and eventually destroyed."

The changes in the spinal column and in the brain described by the Norwegian observers, have not generally been discovered by others, though they are occasionally mentioned.

The destruction of the phalanges by a process of interstitial absorption of the bone, rather than by caries, has been described by Mr. Lisboa, of Bombay, and Dr. Carter.

The Norwegian observers discovered in many of the viscera, deposits considered to be of leprous matter, and Virchow has done so more extensively. This was confirmatory of the old theory of the presence of granular deposits. The laryngoscope has quite recently detected nodules in the chordæ vocales.

Hansen has made out a deposit of large granular brown-tinted cells, not only in the skin and superficial lymphatic glands, but in the liver, spleen, and testes (which old writers used to think especially affected), and in the eye; also in the deep lymphatic glands when the system becomes infected, and thus the constitutional character of the disease is established, that is, the old prevailing opinion, that leprosy was a dyscrasy.

Nothing new has been made out regarding the blood. Danielssen and Boeck's observations about the presence of an excess of albumen and of fibrine have scarcely been confirmed.

Nothing also has been ascertained about the urine, the condition of which was once regarded as one of the tests of leprosy, except that it is often found to contain albumen.

Softening of nodules, and the absorption of their leprous matter into the system, seems to have been carefully watched both in Norway and in India, and the late details on this subject by Dr. Dougall are interesting. The most recent re-

searches of Hansen, alluded to by Dr. Carter, would indicate that he has discovered the presence of a form of bacterium in leprous matter; but the fact, as well as its significance, require to be further investigated. In a general way it may be said, that all the newest information goes to confirm in detail the views that have been most commonly held.

There always has been a difficulty in assigning leprosy its place among diseases. It has a certain analogy with various other cachexias. It was classed with scurvy. Clermon in 1670 writes that scurvy in England was no less frightful a disease to see than leprosy, and no less formidable from its contagion. Men's bodies were deformed no less by it than by leprosy, and whole families were infected with its hereditary contagion. But we know now that scorbutus is essentially a matter of diet, and that to an extent which certainly does not apply to leprosy. Another obvious analogy, which has been especially insisted on by Dr. Milroy, was with struma. Both diseases are often hereditary, and there are glandular enlargements and local deposits in both; but in reality they do not correspond in superficial character or in deep-seated changes. Leprosy has been called a cancer, but it has far more analogy with lupus, especially in its, comparatively speaking, painless ulceration, in its more advanced stages. An old seat of leprosy, Lombardy, presents us with pellagra, which has some points of resemblance in its nervous symptoms and in its burning of the feet. Some forms of ergotism present partial loss of power and gangrene of the toes, but both diseases are essentially different. The notion that syphilis is a form of leprosy now finds few advocates. Leprosy may be complicated with many other affections, such as various skin diseases, with elephantiasis of the Arabs, frequently with syphilis, occasionally with scurvy and with scrofula. Such complications led at one time to a good deal of confusion and to mistaken notions of identity.

The spread of leprosy has been explained by contagion, by inheritance, and by spontaneous origin, at which we shall successively glance.

Contagiousness.—The belief in this was once universal. It was solely the belief in its excessive contagiousness, that led in the first instance to the formation of leper hospitals. The belief in its contagiousness has been dying out. The accounts collected by the College of Physicians, by Dr. Milroy in the West Indies, by Danielssen and Boeck, all point this way. There are scarcely any trustworthy instances of contagion. They are indeed numerous enough, but they do not bear rigorous investigation. Still, in this age, when doctrines of infection flourish with renewed vigour, there is a doubt in the

minds of some, whether leprosy may not be contagious. Dr. Carter evidently suspects that it may be so, but he does not distinctly speak out his suspicions. He says, " Facts are slowly accumulating that show that the accidental inoculation of leprous matter is a way of spreading the disease." It is a pity that he is not more explicit. The belief in the contagiousness of the disease remains strong in eastern countries. A leper is at once avoided. Any mark on the skin that may be interpreted as a sign of leprosy, if detected by his fellow-servants, makes the dismissal of a domestic imperative. In like manner, any such spots appearing on a boy at school necessitate his withdrawal, or that of all the other scholars. Yet, on the other hand, we have known natives eat food prepared by a leper cook, and fruit and sweetmeats freely purchased from lepers.

But popular opinion does not necessarily go far in settling questions of contagion, nor is the opinion of medical men decisive, when we recollect that scurvy was considered both contagious and hereditary, that at one time no one would venture to dispute the fact, and that formerly it was believed that syphilis was communicable by the air. It seems to be absolutely certain, that in no case was the disease ever communicated to the attendants in leper hospitals; and cases ordinarily cited as instances of contagion, may be explained by the fact of such cases having been exposed to the same conditions as those which have given rise to the disease in the subject, from whom they are supposed to have caught it. The cases in which the disease is said to have been contracted by connection with leper women, though they may have been recorded in all ages, are by no means satisfactory or conclusive. Accidental connection with a leper woman was thought more dangerous than cohabitation with her. On the whole, the tendency of medical opinion in the present age is strongly against contagion.

As to its being *hereditary*, most are agreed; indeed, none dispute the influence of descent—some attribute more to this cause, and others less. Carter seems to consider it to be exclusively hereditary, if we may judge by the following expression, " The *rôle* of heredity has been under-estimated, for unless modified by other circumstances, it must be supreme." Yet elsewhere he, like Boeck, expresses the belief in its frequent spontaneous origin. It seems to be hereditary in the same sense as scrofula and gout—it may miss one generation, and appear in the next. Thus, a man born in Europe may get leprosy, and it turns out that his parents were West Indian, who had not themselves suffered, but whose parents, no doubt, had. Like gout, and unlike syphilis, children are never born with it, and they have usually reached the age of nine or ten,

have commonly approached puberty, before any symptoms show themselves. Norwegian writers do indeed mention some few cases of its appearing earlier, even doubtful cases of infants being born with symptoms of the disease on them. The question of heredity gains additional importance in these days, when the avowed object of leper hospitals is to prevent lepers becoming parents.

But far too great importance is ascribed to it, by those who make hereditary transmission even a principal mode of propagating that disease. As Dr. Morehead well observes, seeing that the mortality of lepers is greater than that of ordinary men, that a great many leper women never marry at all, and that thus there are fewer children among lepers than among others, the natural tendency is to the diminution of cases of the disease. Leprosy would have everywhere diminished much more than it has done, unless there were some cause operating to produce new cases, that is, if the disease was not often produced spontaneously, or sprung up anew, when both contagion and hereditary transmission are out of the question.

Whatever importance writers may attach to other causes of it, none, we believe, deny the occasional *spontaneous origin* of leprosy; indeed, we have just remarked that the disease could hardly have continued to exist, unless it sprung up anew. Milroy in the West Indies, Boeck in Norway, affirm that it often arises spontaneously, and even Carter in Bombay says in his reports that the disease "most frequently springs up spontaneously." What are the causes that favour this? Locality, evidently, has some influence. This has been acknowledged by the practice prevalent in all ages, of ordering leprous patients away from the district in which they have acquired the disease. Leprosy is endemic in certain districts, and it occurs frequently in the inhabitants of those regions, even in cases in which contagion or inheritance are out of the question. It may occur, too, in strangers coming to reside in such a district. Europeans, for instance, may, however rarely, acquire the disease by going to live in certain tropical districts; but there is no evidence that a European living at home in a district free from leprosy, can acquire it, except in the rare case of Europeans getting it at home some years after they had ceased to reside in the tropics. This seems to imply a possible latency of the disease. Notwithstanding many limitations, and such facts as the extensive prevalence of leprosy both in the hills and plains of India more than a thousand miles distant from the sea, leprosy is most common along sea coasts and in islands,[1] in this resembling scurvy and Elephantiasis Arabum.

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[1] Intermarriages in a small population may account for the disease being kept up in islands.

The ancients appear to have been aware of this, when they sent their patients to inland places.

It is very desirable that we should know in much greater detail than we do, under what local circumstances leprosy is acquired. The Greeks often attributed it to bad water. In the middle ages the air was generally blamed, and now, to cloak our ignorance on the subject, Dr. Boeck suggests some terrene miasm. Europeans acquire elephantiasis of the Arabs in the tropics, much under the same local circumstances as they do leprosy, but the influence of locality and malaria in producing distinct fever can be proved in the case of the former, but not of the latter. Elephantiasis of the Arabs is, we believe, not transmissible by inheritance. Endemic affections usually are not so, and this, perhaps, presents a presumption against leprosy being a true endemic. Of all causes moist localities have, perhaps, been most blamed as factors of leprosy.

The concurrent opinion of many ages has pointed to rotten fish and salted provisions, as favouring the production of leprosy, and Dr. Milroy, after personal observation in the West Indies, and Mr. Hutchinson, are inclined to attribute a great deal to decayed fish as an article of diet. They predispose, doubtless, to scorbutus and struma, and to cachexias generally, but there is scarcely evidence of their special connection with leprosy, although it was assumed by so early an author as Galen. Bad dwellings, filthy habits, and unfavorable hygienic conditions, doubtless favour the production of leprosy, but it must be remembered that the disease frequently attacks the rich and the well-fed, and is by no means confined exclusively to the poor and ill-nourished.

That unfavorable hygienic conditions exercise an inimical influence on lepers, after the disease has declared itself, and that, on the other hand, the mere amelioration of his hygienic relations without any further treatment improves the condition of the leper, are only facts that are in accordance with all we know of cachectic affections.

Nothing definite can be arrived at from an examination of climate or race. Particular climates and particular races seem to be the favourites of leprosy. But the disease has existed in all climates, and has attacked individuals of every race. It occurs among fish-eating people along the coast of India, and among mountaineers in the Himalayas, who scarcely ever see fish.

If little has been discovered about the ætiology of the disease, very little more has been made out respecting its *treatment*.

The treatment of leprosy has been by general measures, and by medicines used internally or externally.

Kaposi, representing the modern Vienna school, thus sums up the general treatment :—A patient should go from a district where leprosy is present, to one free from it; if possible, he should have a good dwelling and generous diet. He should have baths, according to circumstances warm or cold, sea baths, or vapour baths, and suitable exercise. This is almost a repetition of the words of Aretæus and Cœlius! It seems to be pretty generally admitted, that lepers do mend under change of climate and improved diet—that the progress of the disease is arrested. The purely medical treatment has never been satisfactory. Most writers expect to be able to do a good deal in the early stage of the disease, but not much afterwards. The concurrent testimony of authors in all ages, seems to point to the fact that maculæ and tubercles may in their early stages be made to disappear under careful, chiefly local, treatment; in other words, medicines favour the absorption of the neoplasms of leprosy, at the same time that these formations usually reappear after a time. The list of local applications is endless. Some of the more recent ones are chaulmoogra oil, oil of the cashew nut, and gurjun oil.

It will be very wonderful and contrary to all past experience, if gurjun oil turns out to be more than a mere palliative. Still, the Government of India has shown most commendable zeal in directing that an extensive trial should be made of this new remedy.

Internally, every variety of treatment, chiefly the exhibition of alterative medicines, has been tried with varying success. In all ages there have been special popular cures for leprosy; nothing like a specific cure should commend itself to the judgment of the profession or of governments. Some of the most recent cures have been by Dr. Beauperthuy, in the West Indies, at Bombay, and lately in the Andaman islands, Dr. Dougal has used gurjun oil externally and internally, with apparently striking success. But, after past experience, a much more extended trial of the oil is necessary.

We must now turn to some general considerations deduced from the foregoing premises, which may guide us as to the practical management of leprosy. It is often very difficult to ascertain, whether a disease is increasing or diminishing in a country, and this is specially the case with respect to leprosy. Where there are legal enactments against it, people are interested in denying its existence. It seems probable that the disease is decreasing in Norway, and is stationary in India. The disease, as imported from Norway into the western states of America, seems to be inclined to die out, and that among settlers of French descent in N. Brunswick is certainly not on

the increase. But of the great fact of the nearly total disappearance of the disease from Europe there is no question. The explanation of this is by no means easy. Leprosy possessed at one time a power of spreading in Europe, an attribute which it appears to have lost, and a power which has been only shown by one other chronic disease, syphilis, and that only by certain definite and very different channels. Then, again, in those days, was leprosy really endemic in Europe, as it seems to be in many districts at the present day? If it ever was endemic, when did it cease to be so? As the disease itself is entirely unchanged, just as much so as smallpox, we must presume that surrounding circumstances became less favorable to the propagation of the disease than formerly. In the case of such diseases as dysentery, or the febrile cachexias, we have tolerable evidence that improved drainage, dwellings, and diet, have been the chief causes of their disappearance. We know that scorbutus has been banished by improved diet, and most writers are inclined to suppose that similar agencies may have operated in the diminution of leprosy. Still, it must be remembered, that such agencies have wonderfully little effect on certain diseases presenting some points of analogy, such as syphilis, cancer, or lupus. For these and similar reasons some have regarded the segregation of lepers as having been the most efficient agent in the diminution of leprosy. We must therefore look a little further into the question.

The great majority of writers, both English and foreign, ascribe the diminution of leprosy in Europe mainly to *improved hygienic conditions.* Where there has been great improvement in them, the disease has diminished; where things have remained stationary for centuries, as in Crete, or in the east generally, or in Hindostan, where European civilization has scarcely produced the slightest change in ordinary native habits, there is no material diminution. Leprosy lingered on longest in the parts of Great Britain, which were the last to yield to modern civilisation. The chief argument against this is, that the districts of Norway where there is still leprosy, are not worse off than their neighbours, and that leprosy is not solely a disease of the poor, but occurs occasionally among the rich. But it is not surprising that a transmissible disease should linger on in certain districts; nor, to take the instance of struma, though it is an affection to which those who are well off and well fed are frequently subjected, does any one doubt that good air, good water, good clothing, and good food, have tended greatly to limit its extent.

Besides all this, there is possibly another influence in operation, there is a certain tendency in diseases, even in constitu-

tional ones, to come, and to extend, and to go, which in the present state of our knowledge we are not able to explain. And this tendency may operate in some countries and not in others. Thus, in Europe the fact of the decline of leprosy is certain, while in India it is doubtful. Pliny, as we have seen, told us the disease came to Italy in Pompey's time, and was gone in his. After his time the disease gradually spread in Europe. It seems to have reached a maximum about the time of the Crusades, then died away very much before 1494. A disease, which grew up in Europe within historic times, may very well have had a period of natural decay.

But many attach much greater importance to the effects of *segregation*, than of hygiene or of natural decay. From the earliest ages there has generally been an attempt, to keep lepers separate from the rest of the community. Sometimes this was instinctive, sometimes matter of legal enactment. The earliest measure appears to have been to compel the leper to live by himself outside the city walls. Next, lepers were banished to villages, where they were allowed to live together—have their wives and children with them. At this day this is the case in China and in Burmah, and, indeed, in the Greek islands. When, too, leprosy was dying out in Europe, some of the last remnants of it were found in leper villages in Holland. It is very plain, first, that this amount of segregation is very imperfect, and would afford little protection against the disease if it were contagious; secondly, that it does not interfere with hereditary transmission; and thirdly, we know practically, that when governments in the east have enforced this amount of seclusion, many patients suffering from other affections besides leprosy, have been forced into leper villages, from not having the means of buying off the officials, who threatened to send them to them. But a greater attempt at segregation was made, when leper houses were established. Their original design was solely to guard against contagion, and to remove loathsome objects from sight. Questions of cure, or of the prevention of hereditary transmission, probably entered little into calculation. In theory, at least, during a period of 200 or 300 years, lepers were supposed to be dead to the world; but was there anything like complete segregation even in leper hospitals?

Lepers were always permitted to go about as beggars during certain hours of the day. If they could get husbands or wives to marry them, they were allowed to leave their leper houses. It is evident that they were allowed to travel all over Europe in search of a cure, from shrine to shrine, from medical school to medical school. The great, who were afflicted with the disease, were imperfectly segregated, if at all. It is also generally agreed, that the rules applied to lepers became less strict from the 12th

century, yet their number continued to decrease. It may also be argued generally, that though the amount of segregation was much the same in the different parts of Europe, yet the decline of the disease was not uniform in its rate, and no one has attempted to show, that it bore any proportion to the strictness with which segregation was enforced. Norway had leper hospitals in 1276, and the sending lepers to hospitals was compulsory. In Scotland the laws seem to have been severer than in England, yet the disease lasted longer. The rules in Spain and Portugal were at least as strict as elsewhere, yet the disease is not extinguished. Indeed, the rules for the segregation of lepers have long been stricter in China than in any other country, but there has been no corresponding decrease in the prevalence of the disease. We must therefore look for further causes besides segregation to account for the decline of the disease, and have to fall back on improved hygiene and a tendency in the disease itself to decline, as more adequate causes.

Although no one in these days recommends the revival of Draconic laws such as those of India, which permitted a leper to be buried alive; or those of Scotland, which are said to have ordered castration,[1] and that if a leper woman should have a child, she and her baby should be buried alive; still, there is a tendency to recommend an amount of compulsion, such as has scarcely ever been practically in use. Dr. Carter even hints at "incarceration." To this it may be replied, that there is no proof that stringent rules for the seclusion of lepers, such as exist in some of our West India islands, and as have been enforced by the Dutch in Surinam, have been found to be of any special practical efficacy. Nor in our opinion, do the less stringent rules of Norway, and the experiment at partial seclusion made there afford, as far as we can gather from the Norwegian reports, any very remarkable results, although to others they appear to do so. These measures seem to have been moderately successful, "the improvement not yet very considerable."

But the great argument of those, who employ the popular cry of the day, of stamping out disease—a process, if applicable to any, certainly not to a chronic constitutional affection—is that the decline of leprosy in Europe is owing to the segregation of lepers having prevented the transmission of the disease by hereditary descent. But, in the first place, it has already, we hope, been made apparent that no rules in Europe have been strict enough to interfere with the intercourse of the sexes to any material degree; and, secondly, the fact seems to have been overlooked, that the greatest risk of propagating the disease is, when patients are of leprous habit, and

[1] Castration was an ancient cure for the disease, owing to a belief in the testicles being specially affected.

may have scarcely manifested any signs of the malady. It has always been a question at how early a stage it was wise or humane to enforce segregration. Under ordinary circumstances it was not enforced, till the disease was fairly established. The chances of opportunities for transmitting the disease are obviously greatly diminished, when so loathsome a disease is once established, and the very reverse of the old notion of the presence of satyriasis, is now admitted to be the usual condition of lepers in an advanced stage of the disease. Further, of late years it has come to be clearly established, that there is such a thing as latent leprosy, which may be dormant in one generation, and show itself in the next; against it any possible form of segregation would be inoperative.

To sum up the reasons which make anything like enforced separation of lepers undesirable, we would say that contagion is now believed in only by a few; if the disease is ever transmitted by contagion, the cases are of extreme rarity.

It is an assumption to say, that leper houses and restrictive measures have been the main or nearly the main cause of the decrease of leprosy in Europe.

It is quite an open question, whether the leper houses in Norway are exercising any very striking influence on the decline of the disease. Allowance must be made for what appears to be the natural tendency of the disease to die out in Europe, and to improved hygienic conditions which, however slowly, are yet taking effect even in Norway, according to the appendix to Dr. Carter's report.

With reference to the main object of preventing hereditary transmission, we believe that seclusion would fail, because far the greatest risk of hereditary transmission occurs before a patient is a fit subject for seclusion, and the attempt to ascertain the suspicion of a leprous taint must always be highly inquisitorial. Every one having any practical knowledge of the East will see, what endless power of exaction and of persecution native officials in India would have, if any measure were absolutely penal or coercive. Hospitals now readily sought by lepers, would be avoided. If it were compulsory to go to them, many cases of leprosy would be concealed.

Finally, other diseases, whose hereditary transmission is yet more certain—for instance, insanity, scrofula, and gout, are not subjects of legal enactment, and it is not desirable to single out leprosy, unless the advantage of so doing were placed beyond all possible question. The case of syphilis is obviously different from that of other transmissible affections. Fortunately, though not agreeing with Dr. Carter as to the principle of his recommendations, we cordially agree with him in their practical application, and, so far as they do not recommend

coercion, they are quite in the spirit of the report of the College, to which we are sorry to see that he scarcely pays the respect generally and deservedly accorded.

While not regarding leper hospitals as a means of stamping out the disease, either in their essential nature, or under any circumstance of partial restraint that might be adopted, we strongly advocate their improvement and their multiplication. They help to remove an eyesore from the public view, they promote the comfort of the lepers, and when well managed, tend in a limited sense to check the propagation of the disease. They serve also as schools for the further study of the ætiology and the treatment of leprosy, about both of which there is still so much to learn. Magistrates and district medical officers should be instructed to ascertain the real number of lepers in different districts, and an officer should be appointed whose duty it would be to superintend their inquiries, and give practical advice. A large extent of charity is bestowed in many parts of India, both by Europeans and natives, in a weekly dole to the lepers who come readily enough to receive alms; such charity would probably be better expended in attempts to improve the hygienic condition of the lepers.

It is a gratifying sign of the times, that Government is really inclined to take an interest in the subject of leprosy. The Colonial Office and many colonial governments are taking measures for the improved treatment of lepers. We believe that in Ceylon, the study of that peculiar disease —sometimes mixed up with leprosy, sometimes with syphilis, the Parangay or foreign disease, is encouraged by Government. The various Governments of India take up the question of the treatment of leprosy warmly, and have commended Dr. Dougall's use of gurjun oil to the services. Improvements are taking place in the various small local hospitals at Gyah, in the Punjab, and in the hospital so carefully managed by American missionaries at Almorah, in the Himalayas. The India Office sanctioned Dr. Carter's visit to Norway, and his examination of various leper localities lying more or less on his way back to Bombay. But, after all, it is action rather than further intelligence that we want. We have long had abundant information before us, if we had only acted on it. There is no royal road to the cure or the prevention of leprosy. The work before us is simple enough, but to make it successful, long, steady, and continued action is required. Our measures of late years have been in the right direction, but have been on too limited a scale. They require to be extended; and now that it is ascertained that there are at least 100,000 lepers in India proper, the subject for all practical purposes cannot be overlooked.

IV.—Ziemssen's Handbook of Pathology.[1]

THE desire to bring all knowledge into one scheme and to present it in an encyclopædic form is not especially characteristic of our own age, but is common to all ages of culture. It is, indeed, less rather than more active as knowledge progresses, as labour is divided and as investigation is more varied. To Aristotle or to Pio della Aillirandola the gathering of all science into one whole was to solve the riddle of the universe; it needed but to put feature to feature, and the work of the great Artificer would stand revealed. In modern days such hopes have failed, and if we write "encyclopædias" we do so for purposes of the merest utility, and we expect no more revelation from a shelf of folios than we do from the last pamphlet on spectrum analysis. As the end is changed, so is the method; now it matters little whether the body of knowledge is built up by one hand or by many. No longer is the work advanced by one loving patient seer, who painfully toils onwards towards the day of illumination; but, by one or by many it matters not, are chapters added to chapters and tomes to tomes, until the tale is ended and we find ourselves better furnished, it may be, but no wiser than before. Yet even with hopes no higher than these do we nevertheless bring out systems, conspectuses, dictionaries and the like, in medicine as in all other arts and sciences, and we presume that they serve some good purpose. To collect a bundle of chapters by different hands, to give them no editorial chastisement, to fit them in no way for each other, and to take no adequate care that they shall both share the work duly, and duly cover the whole extent of it, seems to be as useless a piece of toil as ever man bound his hands withal. It is simply to stitch together a lot of magazine articles which have not even that cohesion of thought which may belong to the various writings of a single author. Such a venture may, no doubt, call forth a monograph or two which otherwise would never have seen the light, but surely this is to bring the mountain to Mahomet. So modest a result might have been encompassed by more moderate means. If we are to have a system or an encyclopædia let us have something which pretends to a definite scheme, which shows in all its many parts the print of a governing mind, which is as free from redundance and prolixities on the one hand as it is from awkward gaps and ill-compacted parts on the other, and such a corpus will be welcome and useful. If issued with due rapidity it will present a standard of acquirement characteristic of the

[1] *Handbuch der Speciellen Pathologie und Therapie.* Herausgegeben von Dr. v. ZIEMSSEN. Leipzig, 1874 et seq.

time of its publication, and will embody the history and the extent of our knowledge in a more convenient form than any library of miscellaneous treatises can do. Now, we certainly think that the work which it is our present purpose to notice will satisfy these conditions in greater measure than some others have done which shall be nameless. Four volumes only have appeared as yet, but a very careful scheme of the whole has been issued, and the volumes before us seem to carry out well the promise of the editor, whose name is a guarantee both of ability and of good faith. So far from beginning his "hand-book" as, with a somewhat amusing and very German notion of relative weights, he calls his fifteen octavos, with any casual matter which may first come to hand, and trusting to Providence and the ravens for the future, Prof. Ziemssen, on the contrary, presents us with the following very attractive, comprehensive and well-ordered scheme, which is to be completed in three years. The first volume is devoted to the subject of public health, and it seems a thoughtful arrangement thus to commence a survey of medicine with a consideration of its widest and most general bearings. This first volume contains a treatise on public medicine by Prof. Geigel, a treatise on the diseases of artisans by Dr. Hirt, and a treatise on the inhalation of dust by Dr. Merkel. Dr. Hirt has recently published an exhaustive and able work on the diseases of artisans, and Drs. Geigel and Merkel are also men of the highest reputation upon the subjects they are now called upon to handle. The second volume, which is published in two parts, proceeds by a natural transition to the acute infectious diseases, and here again we find a thoroughly systematic division of this large subject among several physicians all known to us in these inquiries as original and trustworthy authorities. Enteric fever is trusted to Prof. Liebermeister, of Basle, whose investigations into the origin and treatment of enteric fever are so favourably known; Prof. Lebert is no less fitted for his task of describing typhus, relapsing fever and cholera; and so again with Dr. Harnisch, who takes the subject of yellow fever, Prof. Heubner dysentery, Dr. Oertel diphtheria, Prof. Thomas and Dr. Curschmann scarlatina, measles, &c.; Dr. Zülzer erysipelas and miliary fever, and Prof. Hertz malaria; while meningitis cerebro-spinalis epidemica falls to the lot of the accomplished editor. The third volume deals with the chronic diseases of infection. Prof. Baümler, well known in England, takes syphilis, Prof. Heller trichiniasis, hydatids, &c., and Prof. Bollinger the animal poisons, such as rabies, malignant pustule, serpent poisons, &c. The fourth and fifth volumes are to deal with the diseases of the respiratory organs, and these are entrusted to Dr. Fraenkel, the editor and others; phthisis and

tubercle will be described by Profs. Rühle and Rindfleisch. The sixth volume will contain the diseases of the circulatory apparatus, and the seventh and eighth those of the chylopoietic viscera. The ninth is devoted to the diseases of the urinary apparatus, including the bladder, the urethra and the male parts of generation.

The diseases of the female organs of generation will alone occupy the tenth volume, and they are to be set forth by Prof. Schröder. The eleventh and twelfth volumes will be concerned with the nervous system, and will receive the labours of several writers, among the chief of whom are Nothnagel, Hitzig, Erb, Kussmaul and Eulenburg. The thirteenth volume is to be given to the diseases of the organs of locomotion by Senator and Immermann, the fourteenth to diseases of the skin by Rindfleisch and Ziemssen, and in the fifteenth volume will be included all kinds of poisoning. This is a programme as brilliant as it is well conceived and well distributed; nor is it all promise, for in the four volumes before us we have a specimen and an earnest of what is to be. If the coming volumes are like those already issued, we may predict a lasting welcome for the whole undertaking. In his introduction Dr. Geigel tells us that in public medicine there can be little opportunity of saying anything really new, but he trusts that his treatise may find the best kind of novelty in setting forth facts in their due relation and in their proper light. This praise we can certainly give to Dr. Geigel's chapters, which are written in a broad cultivated spirit and with a strong grasp of the facts, both of the present and of the past.

Of the present he says that our age has one remarkable feature, which has, perhaps, never before been seen in any like measure, and that is its conscious tendency to progress to better things, its untiring strife, its daily and hourly march, pressing unresistingly forwards to some wider and better purpose. The awakening of peoples as seen in the revolutions of states, with the consequent destruction of artificial bonds of all kinds and of privilege, the growth of general education and of public morals, and the increase of national wealth and the mastery of capital, are in the writer's opinion the chief features of the new life of humanity. One gruesome menacing spectre alone remains to trouble Dr. Geigel's dreams of the coming time, and to him who cunningly watches is even that one terror hastening to its fall. The Church is the one great unwholesome, the one privileged nuisance which no inspecting has as yet been able to abate. As yet she "ein riesenhaftes, ungastliches Denkmal längst vergangener Zeiten fremdartig in unser grüne Leben hereinragt;" she disturbs both our stomachs and our consciences with her broad-sown hatreds and discords; groaning in the

darkness of her shadow, we shall find no health till she, with her empty "formalisms," her cowardice and her jealousy, has been abolished in the name of healthful morals and culture. We fear she will be tenacious enough to outlive Dr. Geigel, though it seems almost a pity she should after all he has said about her. Dr. Geigel and Humanity having cast a glance round the world together in the introduction, we come to a chapter entitled the conception or idea (Begriff) of public health. This chapter is handled in a bold and able manner, but reminds us at times somewhat of the councilman of New York who could not speak in favour of the paving of a street without going back to the first lines of the constitution. The next chapter deals with the signs or symptoms of the disturbances of public health, and here, of course, are set forth the invaluable aid of statistics and the need of adequate and universal registration of disease. Then follows a chapter on the diagnosis of public diseases, which the author groups under the head of pandemics, of endemics, and of epidemics. The indications of pandemic defects are very properly shown to consist in all those stationary wide-spread prevailing forms of sickness and death which signify a chronic over-strain of certain part or parts of the body during adult life. Then follows the chief section of the treatise, that on the ætiology of the disorders of the public health. The section on public therepautics is, perhaps, equally considerable in every sense, but it is, of course, founded upon the previous thorough inquiry into causation. Dr. Geigel takes the following heads of sections as representing the general "substrata" of public life—air; water; food, which is subdivided into its various kinds, and commerce. These conditions of existence are first considered in themselves and afterwards in respect of their defects and vitiations. It is, of course, impossible for us to attempt even the most sketchy summary of Dr. Geigel's matter; we shall content ourselves with a few references to points of interest. On the subject of food Dr. Geigel is scarcely so able a guide as Dr. Pavy, and we lack in him, perhaps, that thorough familiarity with the recently discovered relations of nitrogenous food to urea and of fats to muscular force which must change our whole conceptions of a sufficient diet and turn us away from the plausible but deceptive opinions of Liebig. It is a good feature, however, in Dr. Geigel's writing that he gives full weight to the practical experience of unlearned men, and allows that universal instincts and practices have a greater convincing power than the most seductive and logical of theories.

Under the head of wine the author urges us to remember that wine is not a mere dilution of alcohol, but a special and highly

complex product, having its own virtues as a cheerer of the heart of man, as the experience of all ages has proved. He reminds us that if without wine the brain be capable of steady continuous work, we may well desire at times to arouse the brain to special activity. Such a temporary exaltation of function may be a great gain to us even if it be followed by a corresponding depression, and a still greater gain if a subsequent depression be not a certain condition, for this, in case of moderate exaltation, it may not be, and indeed seems not to be. To have a brain susceptible of rapid exaltation by such an agent is to possess a more variable and so far a better organization, one more ready to meet the ever-changing needs of life, and a brain so exalted may realise genial ideas and sympathies which are beyond the reach of plodding industry. We give these opinions of the author for what they are worth, in order to show that he does not fail in breadth of view or in manysidedness. The use of tobacco, at first sight seeming so injurious, when studied in the light of experience may admit of defence if not of encouragement, as enabling man so to modify his states as to deal the better with the requirements of his position. Nor must it be forgotten that even the use of mere flavours, by rendering food more acceptable, may thus contribute to improve the power of digestion. Such considerations, says Dr. Geigel, are too often overlooked in formal treatises upon diet. In the section on the commercial conditions of civil life Dr. Geigel properly insists upon the value of work as a direct means of invigorating both mind and body, lack of work having its dangers as well as excess of it or harassing kinds of it. Here also are considered such necessary accidents of commerce as the spread of infectious diseases and the like. In the next chapter we have to consider these conditions of life in their failure or corruption; the influence of open air may be modified for harm by the situation of a city, by the density of its inhabitants, by its manufactures, by the gas used for illumination, by the state of the soil or grouud water, and so forth; while the influence of enclosed atmospheres may be modified for harm by deficient ventilation, by the ill construction of dwellings and public buildings, by defects of clothing, and so forth. Next to air come water, food and luxuries, liable to manifold impurities and the many injurious accidents of commerce.

Under prognosis Dr. Geigel then considers the degree in which these various sources of harm can be neutralised in any given state, and under "Therapie" he shows us how such neutralisation is to be effected. Into all this, of course, we are unable to enter; suffice it to say that we find all the sections treated with a fulness of knowledge and a fertility of comment and suggestion

which are becoming in a book of this pretension. The last two hundred pages of this volume are devoted to the diseases of artisans and to the effects of the inhalations of dust. The special diseases of artisans are taken, as we have said, by Dr. Hirt, whose careful and instructive treatise on the subject is already favourably known to those who are interested in this important inquiry, and Dr. Merkel gives an equally thorough account of the effects of dust upon the respiratory organs. Greenhow's researches in England into these matters are duly acknowledged. There is much in all these chapters over which we would willingly linger did our space permit. We must pass on, however, to give a like superficial account of the second volume—on acute infectious diseases. This volume is published in two parts, and therefore practically two volumes are devoted to this leading subject. Prof. Liebermeister opens with a very closely thought-out and able treatise on enteric fever, which of itself occupies more than two hundred pages. Its chief defect consists in the author's ignorance of any literature but his own. His short "bibliographies" attached to each chapter are both scanty and loosely selected, and with the exception of the names of Murchison and Trousseau, which names could scarcely be overlooked, they consist entirely of German authors. Likewise in the body of the treatise Liebermeister displays an imperfect knowledge of English fever literature, and the names of original investigators such as Ratcliffe, Buchanan, and others, nowhere appear. Now, seeing that the English have both taken the lead and have done by far the best work in fever science, we must regard this limitation of the author's reading as a serious defect. This said, however, we have nothing but praise to give to the treatise. Liebermeister is already well known as an active inquirer into the origin, course, and cure of enteric fever, and we find accordingly that he speaks not as the scribes.

As regards the origin of enteric fever Dr. Liebermeister holds the opinion which we chiefly owe to Budd, and which is shared by the present writer, that this disease does not arise from a mere decomposition of animal or other filth, but that it owes its extension to the continuous growth and propagation of a specific miasm. The distinction is not only one of great scientific interest, but of the highest practical importance. This miasm he believes to consist of minute particles, and not to be gaseous. It is, however, carried into the system both by way of the air and of the drinking water. On these latter points recent experience is so striking—" dass ihnen gegenüber selbst die bewundernswürdige Dialektik eines Pettenkofer nicht mehr ausreichen würde einen Zweifel zu begründen." Indeed, Liebermeister denies that Pettenkofer's observations on the ground

water in Munich give the same results in other places; and he believes that in Munich the apparent or real parallel between its fluctuations and the rise and fall of enteric fever is inessential, and depends upon the common factor of season. It seems clear that summer heat favours the development of the miasm and that winter cold arrests it, and with heat and dryness, cold and moisture, will water-levels vary more or less uniformly. The sketch of the symptoms of enteric fever is condensed and effective, and the long indefinite sickening of some cases and the contrasting suddenness of onset in others is duly noted. The duty of a careful study of the heart is also properly set forth and some interesting pulse tracings are given. For my part, I miss any reference to two points which have long presented themselves to my own mind, namely, the susceptibility of certain families to the contagion as compared with the impunity of others; and, again, the early and alarming disturbances of the nervous system which often arise in persons who possess or who inherit a nervous diathesis. In the section on morbid anatomy the work of Hoffmann and Zenker is, of course, very prominent. Aberrant and deceptive forms of fever allied, or perhaps allied to enteric fever form an interesting and a separate section, and this leads easily to the section on diagnosis. The impossibility of decisive prognosis in typhoid very properly does not discourage the author from a careful estimate of all those phenomena which go to make up the whole tale of probabilities in given cases; but after all, the most experienced physician will say in this treacherous disease "Spera infestis, metue secundis."

The section on treatment is very good and very interesting. Liebermeister has seen nothing to lead him to hope that the disease can be "cut" by quinine, by cold baths, or by any other measures. At the same time means which reduce fever must be so far beneficial when carefully used as to moderate the course of the disease which cannot be arrested. Liebermeister in this section makes some most interesting remarks upon the use of calomel, in doses of 10 and 15 grs.; from this treatment he has seen such encouraging results in hundreds of instances that he regards this drug as among his most useful agents. He gives these doses sometimes twice or thrice in a day (for one day only we presume) with good results. The diarrhœa, at first a little increased, soon subsides in great measure, and the duration and the severity of the disease is modified for good. This opinion is supported by statistics. The author also states that a like modification of the disease seems to be obtained by the use of iodide of potassium; this question is now under investigation in his wards, and he regrets that the results are not yet ready for publication. Dr. Liebermeister's opinions on the use

of the cold bath are too well known to need repetition in this place; we will only say that they do not seem to have undergone any important change, and that they are expressed with becoming moderation and limitation. He commends the conjoint use of quinine and baths, as the quinine makes the need of baths less frequent; he gives the quinine by preference in an evening and in such doses as suffice to bring the morning temperature nearly to the normal, that is, in doses of thirty to sixty grains or more, as the individual case seems to require.

In cases of heart failure, where cold baths are contra-indicated, quinine is of great service, as it will often act as a restorative. It may be injected into the rectum with almost as much success as when given by the mouth, a resource which stands us in good stead where vomiting is troublesome. Under digitalis the author makes a statement which is of the very first importance. In contradiction of the fashionable tendency to administer digitalis deliberately in all cases of heart failure, Dr. Liebermeister urges the very contrary course. Use it, he says, only in cases where the heart is working firmly and where the pulse is moderate in rate. "Sic ist, umgekehrt wie bei Herzkrankheiten, um so weniger indicirt, je excessiver die Pulsfrequenz ist. Die vorbestehende Herzparalyse wird durch Anwendung der Digitalis nicht verhindert, sondern scheint dadurch eben befördert zu werden" (p. 225). The remarks on feeding are judicious; the author approves in general terms of the English revolt against the starving of fevers, but thinks the pressing on of food may at times be done indiscreetly, and he thinks, too, that we often forget to give our patients a sufficiency of water. Our English readers will be astonished to find the small place given to alcohol as a remedy. While on the one hand its use has, no doubt, been inconsiderately urged, yet on the other hand it deserves not to be dismissed in a couple of lines. Prof. Lebert's essay on typhus is altogether a shorter, if not a slighter effort; his brief remarks on treatment show a like reliance upon decidedly antipyretic measures. This eminent writer has reserved his strength for the essay on cholera, which is a valuable contribution to the already so vast literature of the subject, though not, by any means, of the first order of merit. Lebert's interest in the very doubtful fungoid theory of cholera production and the equal doubtful ground water theory of its prevalence has led him to forget to give its full value to the far more certainly known facts of its propagation by drinking water. These are not overlooked, of course, but their relative values are not realised. That cholera can only be kept in check by rigid municipal, national, and international precautions, is very fairly and adequately set forth. Prof. Lebert adds his voice to those

who found whatever therapeutical hopes they may have upon arresting rather than encouraging the diarrhœa. The only chance in Lebert's opinion is to treat the early diarrhœa promptly with opiates and astringents. In a few cases he has seen benefit from a mild laxative, but he thinks that such cases are exceptional.

The articles on plague, yellow fever, and dysentery, appear to be worthy of their place, but my own experience of those diseases is too limited to allow me to express any decided opinion upon their merits ; we may pass on, therefore, to Dr. Oertel's essay on epidemic diphtheria, which closes this part. A better bibliography is prefixed to this essay than to any of the preceding, and the matter is well divided and well put together. Dr. Oertel is well known as an advocate of the parasitic origin of diphtheria which, of late years, has obtained so many adherents, and he makes a clear distinction between diphtheria and croup, though he readily admits that it may be very hard to establish the distinction in given cases. The characters of the exudation are not distinctive, and the physician is driven to compare the general symptoms, the glandular and renal conditions, the epidemic state, and after all may find it impossible to make up his mind. In respect to the treatment, we regret to add that Dr. Oertel's advice is chiefly of a negative kind, for he discourages all kinds of attempts, mechanical or chemical, to destroy the local affection. They are essentially useless and accidentally harmful, and in this we are obliged to agree with him. Dr. Oertel prefers to promote the rapid maturation of the process by inhalations of steam, and to aid the separation of the septic materials by gargling and cleansing. At the same time the symptoms of general poisoning can be dealt with only on general principles. If the editor can gather together essays as good as this study of diphtheria, his "hand-book" will take a high place in contemporary medical literature.

Taking the volumes, not in the order of subject, but in the order of their publication, and this for obvious reasons, we have to leap over the second part of the second volume, and to notice in the next place the third volume, which is devoted to the chronic infectious diseases.

It is not easy to give any of the chief conclusions of Dr. Bäumler's article on syphilis, as the article is rather a careful digest of current opinions than a record of personal experience. Dr. Bäumler has taken the course, and no course could have been better on this occasion, of dividing his subject into separate heads, of prefixing careful bibliographies to each, of devoting impartial care to every one of them, and of aiming at something as near completeness in treatment as his space would permit; and turning over these pages we find no opinion or

discovery of importance omitted; we find Hutchinson, Bumstead, Ricord, and other foreign writers, as well known to the author as his own countrymen. We find conflicting opinions judiciously and concisely compared, and we find the historical and other accessory chapters quite equal to the rest. The essay has evidently cost its writer great care and reading in its preparation.

Dr. Bäumler adds nothing to our knowledge of the early history of syphilis, but he gives an amusing reason for the long-lived belief that impurities of the atmosphere were the cause of the disease. "Verderbniss der Luft wurde von Anderen als Krankheitsursache angenommen, und, hauptsachlich wohl um das Erkranken von Mönchen und Nonnen zu erklären oder hochgestellen Personen, namentlich geistlichen Standes, zu Gefallen betrachtete man die Luft als Vermitslerin der Austeckung" (p. 6).

The syphilitic affections of the nervous system are only touched upon in this article, the full consideration of them is left for the volume on nervous diseases. The therapeutical sections are written in a judicious and adequate manner, and represent the practice which is now almost universal among the best physicians. Dr. Heller's essay on echinococcus, cysticercus, and trichina, is complete and concise, and it is sufficiently illustrated with good and original woodcuts.

Dr. Bollinger concludes this volume with a long and apparently excellent study of the various animal poisons by which man is liable to be affected.

We can only say that these latter essays seem to be excellently carried out, because our own experience of many of them is very limited. Snake bites are, fortunately, almost unknown in England; and glanders, the "foot-and-mouth" disease, and malignant carbuncle, as they occur in mankind, are rather matters of hearsay than of experience in England. Still, these subjects are treated in an interesting way, and are in a handy form for reference when occasion arises.

We regret to find that the author cannot give any opinion as to the asserted success of Halford's treatment of snake bite, which is disbelieved by the Indian authorities, so far at least as the Indian snake poisons are concerned. The chapter on hydrophobia, on the contrary, deals with a historical disease, which of late has only been too frequent, and which in its mysterious and sudden invasion and its terrible and fatal progress has a horrible fascination for the public and an awful interest for the physician. Dr. Bollinger does not enter into the very anxious question whether the poison of rabies may be developed "spontaneously" in carnivora, under conditions of fury or of disease of other

kinds, as suggested by Dr. Muscroft of Pontefract. The poison, both in the site of its formation and in its physiological characters, reminds us of snake poisons, which have undoubtedly been developed by variation from normal salivary secretions. However this may be, the advance of the disease is mainly by infection, and its prevalance must always be a disgrace to a community. Rabies has, undoubtedly, become more common in England since the dog-tax was lowered; a dog-tax of ten shillings, and a rigorous enforcement of the tax, would, no doubt, soon extirpate the disease. Meanwhile foolish mayors issue placards in ignorant fear of the "dog-days," when they make a spasmodic raid upon unmuzzled dogs and slumber for the rest of the year.

In the section on symptoms Dr. Bollinger divides rabies into the raving and still forms (rasende Wuth und stille Wuth). The raving form has three periods—1, the melancholy stage; 2, the maniacal stage; 3, the paralytic stage. The still form consists in a prolongation of the first stage, including the alteration of the voice, and in a transition from this to the paralytic stage without the intervention of mania. The author does not doubt, of course, the identity in nature of the two forms of rabies, but rightly thinks the clinical distinction to be convenient.

Under the head of pathology the author is less well informed. He gives no results of microscopical examination of the central nervous system, and has overlooked the important observations herein of Meynert, of Bastian, of Hammond and of the present writer. Under diagnosis, again, while such stupid blunders as that which led a short time ago to the brutal slaughter of an epileptic mastiff in Fleet Street are provided against, yet we find no allusion to the opinions of those who, like Grantley Berkeley, assert that there is a true and a false canine rabies, the latter being harmless to man.

The second part of the second volume was published after the third volume, which must excuse our dealing with it out of its number. The second part deals with chicken-pox, measles, "rotheln," and scarlatina, which are handled by Prof. Thomas, of Leipzig; with smallpox, which is entrusted to Dr. Curschmann, of Berlin; with erysipelas, sweating sickness, dandy-fever, influenza, and hay fever, by Prof. Zuelger, of Berlin; with malaria, by Prof. Hertz, of Amsterdam; and finally with epidemic cerebro-spinal meningitis, which is described by the editor himself.

Of this last section I can only say that the reputation of v. Ziemssen, the admirable and comprehensive bibliography, and the thorough style and argument, seem to indicate this as a standard essay on this terrible and mysterious subject. As no

case of the kind has ever occurred within my own experience, I am unable to pass any more formal judgment than this. Probably most readers will turn in the first instance, as I did, to Prof. Thomas's article on scarlatina, as this protean malady is a severe test of clinical and pathological knowledge. Prof. Thomas, however, describes scarlatina in a way worthy of the famous and progressive school of which he is a distinguished ornament; accurate, compact, complete, and informed by personal observation and thought, the article is a model of what such an article should be. Every section is adequately worked up, unless it be that the historical is somewhat curt. Although scarlet fever is not definitely known to have existed before the sixteenth century, yet many writers have collected evidence in favour of its occurrence in earlier ages of the world. The late Dr. Collier went so far as to write a treatise purporting to prove that the plague of Athens was scarlatina, and the words of Thucydides lend themselves to such a notion more readily than might be supposed. Under ætiology Prof. Thomas describes inoculative experiments, examinations of the blood, also the spreading of the poison by means of milk. The contagion of scarlatina seems to him to be less volatile than that of measles, and he supports the practice of free ventilation as an efficient means of lessening the intensity of the poison. Indeed, he is disposed to think that a free mixture of external air with that of the sick-room so far dilutes the poison as to make it harmless. This I doubt, as one outbreak of scarlatina in my experience was due to the wafting of the poison from clothes in the open air through the windows of a hospital. With one remark I heartily agree, namely, that there is no evidence worth citation to prove the common belief that contagion continues during and ceases with desquamation. It is undoubted that contagion is less active as convalescence is established, but when it ceases it is impossible to say. Prof. Thomas notices the extreme susceptibility of certain families to the scarlatinal poison, but he does not refer to the hypothesis held by the present writer, and probably by many others, that the immunity given by its occurrence may extend beyond the life of the individual and give protection to the descendants. It is thus that we may explain the virulence of this class of infectious diseases on their first introduction into new countries. On another point the author supports a growing opinion, namely, that the prevalence of scarlatina does not depend wholly upon the mere propagation by contiguity, but that certain states of soil favour the activity of the poison. Thomas thinks the "extraordinarily strong disposition to the disease in England" may be due to conditions of soil. Marchioli,

quoted by Thomas, says that Recorsano, which lies in a swampy district, suffered exclusively and terribly from this disease, and two remarkable instances of a like kind have occurred in my experience. In a West Riding village, where I once saw a sporadic case of scarlatina, the medical man told me that a certain part of the village, which at one time lay near a marsh, was then never free from scarlatina, but that since this marsh had been drained and cultivated the disease had almost disappeared. The second instance—a small town in the eastern counties—was like the former in all respects save one, namely, that the marsh has not been drained and that scarlatina has there a permanent home. The prolonged oscillating course of scarlatinal epidemics is, however, always in contrast to the rapid culmination and fall of epidemic measles. There are many paragraphs in the thoroughly good chapters on the pathology and symptoms of scarlatina which I pass over with regret, there is so much to invite attention. The chapter on treatment is, with the author's wonted thorough intelligence, introduced by a careful review of our means of prophylaxis. The medicinal antidotes which have been proposed, and of which belladonna is the chief, are dismissed in a few lines as unworthy of discussion. Specific treatment in like manner he regards as visionary, and points out that all rational treatment, as yet known, is non-specific.

Hydrotherapeutic methods are cautiously discussed and recommended ; quinine in gramme doses and digitalis also are to be used in combating pyrexia and frequent pulse ; camphor, musk, ammonia and alcohol, in warding off exhaustion. For the angina ice and mild detergent gargles are in ordinary cases preferred to more energetic means. Did our space permit we would gladly extend our analysis of this article, and we would deal with the rest of the volume in like manner, for no one of the articles is inadequate, though some seem better than others. Hay fever is rather oddly inserted among the acute infectious diseases ; but, no doubt, it presents important analogies to them. Blackley's researches are duly recorded and considered, and, like Blackley, Zuelger considers that treatment is powerless in this troublesome affection. Flight offers the only chance to the susceptible. One serious oversight there is in this article, namely, that under the head of the diagnosis no mention is made of the distressing coryza and spasmodic sneezing to which many people are subject when exposed to sunlight. Such persons are thrown into paroxysms if their blinds be drawn up of a summer morning, and will sneeze violently and repeatedly in passing from shadow to light when walking abroad. Such persons often believe that they suffer from hay asthma, as, for obvious reasons, their attacks

occur in summer, but a little observation will show that they may cross a hay-field on a dull day with impunity. Such persons are readily cured by arsenic, and hence the credit given by some to arsenic as a remedy for hay fever. The recently proposed plan of curing hay fever by the local use of quinine finds no favour with Zuelger.

Here we must suspend our review of this interesting work, as no farther volumes have issued from the press at the time of our writing, We look forward with great interest to its continuance, as we expect to find in it the best expression of the medical science of our day.—T. C. A.

IV.—Recent Physiological Works.[1]

All who pay attention to the progress of physiology must have noticed the rise and fall in the degree of interest attached to particular departments of this vast subject. How certain points appear for a time to attract the special attention of the principal workers, whilst others are comparatively neglected. How after a period of oblivion some organ or tissue suddenly comes to the fore, is tortured in a thousand ways, is the subject of innumerable experiments, is investigated by the microscopist, by the chemist, by the physicist, till, as much insight into its structure and functions having been gained as the means of investigation in use at the time allow, it once more falls into the background, to become prominent again when the general advance of other branches of science and ripening of knowledge permit old quarries to be reopened and worked to advantage. Then facts and phenomena previously

[1] 1. *Leçons sur la Physiologie normale et pathologique du Système nerveux.* Par Dr. Poincaré, Prof. adjoint à la Faculté de Médecine de Nancy. Paris, 1873.

2. Dittmar, *Die Reizbarkeit der Centripetalen Fasern des Rückenmarks,* in 'Ludwig's Arbeiten,' 1871.

3. C. Dittmar, *Ueber der Lage des sogenannten Gefäss-Centrums in der Medulla Oblongata,* 'Ludwig's Arbeiten,' 1874, Band viii, p. 103.

4. V. Braam Hougkeest *über die Peristaltische Bewegungen von Magen und Darm-canal,* in 'Pflüger's Archiv,' Bd. viii, p. 163.

5. *Untersuchungen über das Gehirn.* Dr. Eduard Hitzig. Berlin, 1874. Pp. 276.

6. *Experimental Researches in Cerebral Physiology and Pathology.* David Ferrier, M.D., in the 'West Riding Lunatic Asylum Hospital Reports,' vol. iii, pp. 30-96.

7. Nothnagel, 'Virchow's Archiv,' B. lvii, p. 184; lviii, p. 420; and lx, p. 128.

8. *Études de Physiologie et de Pathologie Cérébrales.* Par J. Luys. "Des Actions reflexes du Cerveau." 1874. Pp. 200.

unintelligible are at once co-ordinated and the whole science receives a fresh impulse to its extension and development.

It is thus that at different times a powerful stimulus has been given to the study of the process of digestion by the publication of the investigations of Spallanzani and Reaumur, the observations of Dr. Beaumont on Alexis St. Martin, the chemical researches of Meissner and Brücke on the conversion of albumen into peptones, the microscopical observations of Funke and others upon the structure of the intestinal epithelium, the discovery of the nervous plexuses of Auerbach and Meissner, and the researches of Bernard and others on the pancreas and intestinal juice, which have constituted so many epochs or steps in the history of that particular subject. So, again, the anatomical inquiries of Kiernan, the microscopical researches of Beale, the chemical experiments of Bernard, the microscopical examinations of Hering and Chrzonsczszewsky, have successively attracted the special attention of physiologists to the structure and functions of the liver; and we might adduce other and equally marked instances in the case of the kidney, of muscular tissue, and of the development of the ovum.

The nervous system has not been exempt from these phases of interest and comparative neglect on the part of the great body of physiologists, though on the whole it has attracted more and more persistent interest than any other tissue or organ of the body.

The causes of this are very apparent. Its softness and liability to change after removal from the body or from the effects of reagents render it a very difficult subject to examine, and it has only been by degrees that the wonderful intricacy it presents has been unravelled. Chromic acid and bichromate of potash, iodized serum, and other hardening reagents, and especially the use of Müller's fluid, have been successively proposed and largely employed, and have proved of immense service in aiding investigations into its structure. Then the complexity of its chemical composition, though it has up to the present time foiled the efforts of the most practical analysts, has yet led to numerous researches with each advance of chemical science; and, lastly, the discovery of the various methods by which the functions of particular nerves and centres may be ascertained, as by tracing the centric and peripheric relations of the former, by irritation applied to their distal and proximal cut surfaces, by microscopical examination after division, by observation of the effects of poisons, and by the researches of the comparative anatomist, have all in succession attracted the attention of numerous observers. Thus it is, that new questions of interest arise in apparently endless succession, each throwing light on others and

aiding the more perfect comprehension of what was previously obscure. Moreover, the nervous system presents many points of contact with other branches of inquiry. The physiologist here meets on common ground with the metaphysician, the physicist, and even with the mathematician, and all can afford him aid and counsel in the kind and method of experiment he should adopt.

Few subjects have received a greater amount of benefit from the development of new and delicate methods of research than the study of the nervous system; nor, indeed, is there any which better repays the pains and thought, however great, that are bestowed upon it. To discover the connection between psychical and physical powers, to translate terms of mind into terms of matter, to express a thought by a chemical formula, has not hitherto and probably will never be accomplished; still, it is something to be able to measure the rapidity with which a nervous impulse resulting in a muscular act traverses the nerves, something to have ascertained the laws of transmission of such impulses, and the duration of the simplest exercise of the reasoning powers.

To obtain these results, however, very delicate instruments of research are needed, and cylinders or discs revolving with great rapidity, a series of levers adopted to register the minutest movements, chronographs to record periods of time so short that they are measured by the almost imperceptible vibration of the tuning-fork, have superseded the coarse methods of observation at the disposal of physiologists but a few years ago. And it must be added that the skill in devising and the manipulative dexterity in executing many of the experiments which have led to recent results are worthy of all praise. The very complexity of the arrangements for such experiments, however, naturally leads to more or less discrepancy in the results obtained, and we accordingly find that the conflict of opinion in regard to many points that were formerly generally admitted is considerable.

It is impossible to deny that, to fathom the physiology of the brain, experiments on living animals must be performed; and without entering into the discussion in regard to vivisection that is at present taking place, it may fairly be said that whilst it is highly important that no unnecessary cruelty should be inflicted on animals, it is absolutely necessary that certain well-considered experiments should be performed on the living animal if any material advances are to be made in our knowledge of the functions of the brain and nervous centres generally.

To experiments of this nature we shall have occasion to refer in the subsequent part of this paper, and we shall only here stay to remark that we do not think, so far as we our-

selves have observed, any unnecessary cruelty has been practised by experimenters in this country, certainly none that could compare with the pain that is inflicted for other than scientific objects.

With these preliminary remarks we proceed to consider M. Poincaré's work, which consists of a series of lectures delivered in 1873 in the School of Medicine at Nancy, and which constitutes a very complete exposition of the present state of our knowledge in regard to the normal and pathological conditions of the nervous system. The subject is discussed in the following order:—1. General facts in regard to the different modes of manifestation of the innervation, as by sensory, motor, nutritive, and psychical actions. 2. The anatomical structure of the spinal cord. 3. The results of direct irritation of the different parts of the cord. 4. Phenomena of transmission of motor and sensory impulses of which the cord is the seat. 5. Laws of reflex action. 6. Part played by the cord in the different functions of life. Lastly, the author considers the cord in its abnormal conditions. A considerable number of woodcuts are intercalated, which, though rough, are very suggestive, and we must regret that our space will not allow us to reproduce them here. The first few chapters of M. Poincaré's treatise deal with well-known matters, to which it is unnecessary to make any reference. In the fourth chapter he has collected with much care the views of fifteen of the best-known physiologists upon the functions of the several columns of the spinal cord, and shows that, although there have been and still are some differences of opinion amongst them, the general tendency of modern investigations has been to prove that—1. The direct irritation of the *posterior columns* causes *movements* which are due to the action of the fibres proper to their columns, and signs of *sensation* which are exclusively due to the posterior roots which traverse these columns. 2. Direct irritation of the anterior columns, if violent, causes movements and slight evidence of sensation due to the presence of sensory recurrent fibres. 3. The grey substance is perfectly inert, notwithstanding that it is destined to conduct sensory impressions. It is not improbable, in view of the recent researches on the brain to be hereafter mentioned, that this statement will have to be modified, and perhaps the proposition should be,—The grey substance, with our present means of applying irritation, is perfectly inert. M. Poincaré proceeds to give the details of Brown-Séquard's experiments to show the mode in which sensory impressions are transmitted, with illustrative sketches, and states his own opinion to be that sensory nerves entering the cord by the posterior roots pass up or down the cord for three or four centimeters, and then enter the posterior cornu of the opposite half of the cord, after which they distribute themselves through

the demizone of the grey substance and reach the brain in this state of diffusion.

It does not appear that the more recent method, chiefly pursued by Brown-Séquard, of determining the function of a given column of the cord by suppression or destruction has led to results that are materially different from those obtained by the older plan of irritation adopted by Longet; however, it may be as well here to give briefly the results of sections made through the several columns of the cord. In the first place if the *posterior columns* of the spinal cord be divided in the lumbar region, the animal can still perform all voluntary movements of the posterior limb, though with some loss of co-ordination of the several muscles. Secondly, if the *antero-lateral columns* be divided, it will be found that all the muscles that receive their motor nerves from the parts of the cord below the section remain completely motionless. These columns, then, it is evident, transmit the mandates of the will to the muscles. Thirdly, if a section be made through the posterior column and the grey substance, without injuring the antero-lateral columns, the voluntary power over the muscles is weakened, and sometimes is completely abolished. Calmeil, v. Deen, Stilling, and Brown-Séquard, held it as proved that, if the antero-lateral columns played this important part in the transmission of motor impulses, the grey substance was also influential. The results of anatomical investigation, however, show that the reason why section of the antero-lateral columns alone is followed by complete paralysis is, that the motor fibres run through them to the motor cells of the anterior cornua of the grey substance, whilst from these cells fibres are given off others which ascend, overlying one another in an imbricated manner, to reach the brain. Hence a section through the antero-lateral columns divides all the ascending fibres, whilst section of the grey matter only destroys the central motor action of such cells as are comprised in the artificial lesion, those that are below still remaining in relation with the brain, because the fibres originating from them have already entered the antero-lateral column. The feebleness that results from such section of the grey substance is probably dependent upon loss of sensation. Of the two columns which together constitute the antero-lateral column in the dorsal region the anterior plays the most important rôle in the transmission of motor impulses to the back, as may be shown by the great diminution in muscular power that results from its section, and the small amount of diminution that results from section of the lateral column, providing the anterior column is preserved. It is remarkable that the opposite holds good in the cervical region. M. Poincaré passes in review the simple reflex acts resulting from irritation of the spinal cord, and

points out that, although there is free communication between the cells of the cord, making it a single organ, it is nevertheless easy to demonstrate by sections that reflex movements can be effected by successive discs of its substance when perfectly isolated; and that in this point of view it is comparable with the gangliated cord of the articulata.

He then gives the laws that govern reflex actions laid down by Pflüger, and these are so clearly summarised that we are tempted to reproduce them here, especially as, so far as we know, they have not appeared in their integrity in any English work, though, of course, the principal facts have long been familiar to all.

The first is the *law of unilateral reflexion*, which is that when reflex movements only take place on one side it is always on that side on which the sensory impression has been made.

2. *The law of symmetry.* This is that when the impression radiates so that movements are produced on the other side, the muscles that are moved are always the congeners of those of the side directly acted on.

3. *The law of intensity.* In some few instances the congeneric movements are equally vigorous, but, as a rule, the movements are most pronounced on the side which has received the irritation.

4. *The law of reflex radiation.* In the spinal cord the motor nerve primarily affected is on the same plane as the sensory root that has been excited. If the effect extends beyond this point it affects the motor nerves situated *above*, and never those *below*. In a word, radiation invariably takes place from below upwards. The reflex movements due to the activity of the encephalon, on the other hand, radiate from above downwards towards the medulla oblongata, which segment of the cerebro-spinal axis has the power of generalising all reflex movements. Whenever any radiation in its ascent through the cord reaches the medulla oblongata all the nerves which emanate from the encephalon take part in the excitation.

5. The sensory nerves and the motor cells are so disposed that each reflex movement is perfectly adapted to react against the impression received. Thus, if the foot of a frog be pinched the flexors of the leg and thigh act to withdraw it; if the pinch be more violent both legs are drawn up, and then the extensors act, so that there is a veritable locomotion, the animal, in other words, taking refuge in flight. If a drop of acid be placed on the anus both legs are brought up to brush it away. Volkmann has observed similar phenomena in dogs. Auerbach has recorded a still more curious case. He cut off the leg of a decapitated frog at the thigh and placed a drop of acetic acid on

the corresponding side of the back. The animal attempted to reach the point irritated with the stump, but after many efforts gave it up as a thing impossible. Auerbach then placed another drop on the opposite side of the back. The frog immediately swept the place with its intact leg. But then followed this remarkable phenomenon—that, as if success had given rise to an idea that had not previously existed, it appeared to recognise that the healthy foot could be used to rub the point first moistened with the acid, and it did so.

6. The manifestations of the reflex power of the spinal cord are much more intense when it is separated from the encephalon, and this intensity augments in proportion to the shortness of the segment.

7. The intensity of the reflex power varies with the species of animal under experiment; birds, in this respect, occupy the highest rank, then mammals, reptiles, and fish.

8. The younger the animal the more marked is the reflex activity of the cord. The frequency of convulsions in children from slight causes is in accordance with this law.

9. Reflex movements can be much more readily induced by irritation of the peripheric extremities of sensory nerves than of their trunks.

10. Peripheric regions are not all equally apt to provoke reflex movements. The places of election for it are for the skin, the palm of the hand, sole of the foot, and the hollow of the axilla; and for mucous membranes, the conjunctiva, the pituitary body, and the mucous membrane of the fauces.

11. The nature of the excitant is of considerable importance. Witness the effects of tickling as compared with firm pressure.

12. The reflex action of the spinal cord is exalted by certain substances, amongst which opium, alcohol, nicotine, ether, chloroform, and above all strychnia, may be mentioned.

13. On the other hand, there are substances which lower the reflex action of the spinal cord, and amongst them hydrocyanic acid, belladonna, and bromide of potassium, may be enumerated.

14. The reflex power augments in certain states of disease, as in all inflammatory affections, in septicæmia and icterus.

15. The reflex power can be suspended by exhaustion.

16. And lastly, the quantity and quality of the blood supplied to the cord exercises a marked influence on its reflex power.

These constitute the laws of Pflüger, and are deserving of careful attention.

We are unable to follow M. Poincaré through his consideration of the part played by the cord in the different functions of the economy, as the respiration, circulation, digestion, generation, &c., though these are well and interestingly examined.

The remainder of M. Poincaré's work is occupied with the pathology of the nervous system, in which he points out the various changes nerves undergo after section and the symptoms presented after lesion of the cord, of which he gives a capital and very instructive table compiled from and contrasting the observations of Monod, Oré, Gendrin, Chélius, Vigués, Boyer, Brown-Séquard, Dundas, Russell Reynolds, Radcliffe, Jackson, and many others, and shows how the pathological evidence supports the results of experimental inquiry in regard to the course of the motor and sensory fibres in the cord. He then proceeds to consider the relation of various diseases to certain well-defined and limited lesions of the cord. Thus, infantile paralysis results chiefly from destruction or atrophy of the cells of the anterior cornua of the grey matter, as does also the progressive muscular atrophy of adults, whilst chorea, so far as the spinal cord is concerned, is chiefly associated with disease of the posterior cornua. In connection with this he mentions a case that accoucheurs who are fond of giving ergot in large doses will do well to bear in mind, in which the most violent and persistent attack of chorea resulted from an overdose administered in a difficult labour. He associates tetanus with chorea as consequent upon lesion of the posterior columns, though admitting that both conditions embrace ultimately almost all the regions of the cerebro-spinal axis with the exception of the cerebral lobes. He describes the symptoms of locomotor ataxy at considerable length, and refers it to lesion of the posterior columns. Finally, he deals with the several forms of paralysis. M. Poincaré then proceeds to discuss the functions and diseases of the medulla oblongata, in the same exhaustive manner as those of the spinal cord.

In Dittmar's experiments on the excitability of the centripetal fibres of the spinal cord, the variations in the blood pressure were regarded as the indication and measure of that excitability, and his observation fully corroborates the statements made by V. Bezold, that the slightest impressions made on sensory nerves, even upon the auditory nerves, are responded to by an increase of blood pressure, and this even when the animal is under the influence of woorara and when both vagi have been divided; that is to say, when all muscular movements that might arise from pain are abolished and all influences excited by the vagi are prevented. The increase of pressure in these cases is undoubtedly due to reflex action of the vaso-motor nerves upon the vessels causing their constriction. The disturbing influence of variations in the respiratory activity, which had proved so great a stumbling-block in previous experiments, was overcome by Dittmar by means of mechanical apparatus, with the aid of which artificial respiration could be maintained so regularly that the

blood pressure continued unaltered for hours. Amongst the more important results obtained by Dittmar were, first, that irritation applied to the spinal cord at the point of entry of a sensory nerve—say one of the intercostal nerves—produced a much greater effect than irritation of the nerve with a stimulus of the same strength. This does not, indeed, prove that the spinal cord is excitable, because the nerve-fibres of the nerve might have been and probably were irritated. On longitudial division of the cerebral end of a transversly divided cord into an anterior and posterior half, irritation of the anterior half produces a greater effect than that of the posterior half, but each causes increase of blood pressure. Dittmar found incidentally that the dura mater was excessively sensitive, any injury inflicted upon it causing increase of blood pressure. His principal reason for regarding centripetal fibres of the spinal cord as excitable is, however, the following experiment. An animal is placed under the influence of woorara and a manometer is connected with its carotid artery. The spinal cord is laid bare throughout a part of its length and divided at the lower border of the wound. The dura mater is opened up, and by means of a fine and sharp knife a longitudinal section is made just in front of the points of entrance of the two posterior roots, so that the posterior portion includes the posterior columns and the posterior horns of the grey matter, and the other the greater part of the grey substance with the antero-lateral columns of white matter. The anterior roots are now torn through close to the cord with a needle. It can now be shown that the anterior longitudinal half of the cord, wholly separated from the posterior roots, is one of the most sensitive structures of the body, the slightest excitation, mechanical or electrical, causing an immediate elevation of the blood pressure. Further division of the cord longitudinally into anterior and lateral portions, with a view of determining by which of the two the impression, were chiefly conducted, was not followed by very satisfactory results, and the experiments were too few to permit any conclusions to be drawn.

Amongst the subdivisions of the nervous system none have attracted more attention of late years than the system of nerves now known as the vaso-motor nerves, and our readers will see some of the principal recent papers quoted at the head of this article.

Both experiment and clinical observation have long taught us, says Prof. Dittmar in his article on this point, that the central organs of several highly important physiological and pathological processes must be locked for in the medulla oblongata, amongst which may especially be noted those for the respiratory movements, and for the regulation of the con-

traction of the vessels. As the excitation of the vaso-motor centres causes contraction of the smaller arteries, which practically prevents the discharge of the blood from the arteries into the capillaries and veins and thus effects an increase in the tension of the blood in the larger vessels, and as slight variations in this condition are recognisable by well-known mechanical apparatus, we have at hand a ready means of determining the exact site of these centres by carefully made sections and punctures in animals rendered motionless by woorara. Owsjannikow, pursuing this method, determined the lower limit of the vaso-motor centre at 4 mm. above the calamus scriptorius, whilst the upper limit was about 4 mm. ($\frac{1}{6}$ inch) higher, or from 1 to 2 mm. below the corpora quadrigemina. Dittmar has suggested several improvements in the mode of effecting the necessary sections upon the living animal by adapting a piece of brass with a slit in it to Czermak's rabbit-holder. The knife is thrust through the slit, and all obliquity or other irregularity of the section is avoided. His experiments have in the main corroborated those of Owsjannikow. He finds the lower limit of the vaso-motor centre about 3 mm. above the point of the calamus scriptorius and 1 to $1\frac{1}{2}$ min. below the lower border of the tuberculum laterale. The upper limit lies in the vicinity of the fovea anterior, or at about the upper border of the corpus trapezoides. The space so marked out corresponds almost precisely to the territory of origin of the facialis. The mutual relations of the nucleus having been thus made out, Dittmar set himself to determine the sectional area of the cord that it covered. The observations of Miescher and Nawrocki[1] had already pointed out that the centripetal fibres of the sciatic nerve, the irritation of which caused reflectional contraction of the vessels, ran in the lower regions of the spinal cord in the lateral columns. Dittmar repeated these experiments at the level of the third cervical vertebra, and found that destruction of the anterior and posterior columns as well as of the grey substance of the cord, even at that height, did not interfere with exaltation of the blood pressure on irritation of the sciatic. It follows, therefore, that not only the centripetal fibres of the sciatic, the excitation of which causes increase of blood pressure, but also the vaso-motor nerves themselves, must run in the lateral columns of the cord. Further, very carefully made partial horizontal sections showed that the remains of the anterior columns and pyramids, as well as the remains of the posterior columns and the posterior part of the remains of the lateral columns, could be removed or destroyed without disturbing the vaso-motor reflex action. There remains, therefore, on each

[1] 'Ludwig's Arbeit.,' 1870.

side only a small prismatic space within the vertical limits of the vaso-motor region, the destruction of which abolishes reflex vaso-motor action. The part that remains so endowed is the continuation through the medulla oblongata of the anterior part of the lateral column.

The question of the mode in which the nervous system acts in effecting dilatation of the vessels, or, in other words, the subject of the existence or non-existence of special dilator nerves, is one of great interest, and cannot be said to be yet quite definitely settled. On the one hand some excellent physiologists, Schiff among others, hold that there are certain nerves which have a direct dilating action on the walls of the vessels, just as other nerves may be regarded as effecting contraction. Other no less eminent physiologists, as M. Brown-Séquard, think that the dilatation is a secondary phenomenon, and results from the suractivity of the anatomical elements of nerve, muscle, gland, or other organ, which has been primarily excited by the stimulus applied to the nerves. M. Claude-Bernard regards the phenomena presented as analogous to those known in physics as phenomena of *interference*, and that they are dependent upon the action of the vaso-dilator nerves upon the vaso-motor nerves. M. Vulpian's inquiries have led him to take up a somewhat similar position. For him the vaso-constrictor nerves are in a state of constant activity, as a result of which the vessels are maintained persistently in a condition of slight constriction, ordinarily recognised as *tone*. When this influence or state of tonic contraction is removed the vessels dilate. Such removal is effected by division of the vaso-motor nerves, and it is therefore clear that these conduct impulses from certain centres in the medulla oblongata, and perhaps elsewhere, which bring about the contraction in question. There are, he believes, ganglionic centres in the course of the vaso-motor nerves in which the dilator nerves terminate, and when these latter are in action they so modify the molecular disposition in the ganglia that the vaso-motor nerves cease to exert their influence. As a consequence the vascular tone is diminished and the vessels dilate to an extent proportionate to the strength of their walls, the external pressure to which they are subjected, and the pressure of the blood in their interior.

It may be well to add that V. Braam Hougkeest's experiments seem to demonstrate that the inhibitory action of the splanchnic nerves upon the intestines is not due to their influence upon the vessels. This he has shown by immersing the animal in a warm solution of common salt and exciting the exposed intestines to vigorous movements by irritation of the vagus. The splanchnics were then divided and a very weak stimulus

applied to the distal portion of the trunk. The effect observed was immediate arrest of the movements, the intestines remaining of their natural colour and not becoming pale as they would have done if the vessels had contracted. The stimulus employed was sufficient to excite the inhibitory, but not the vaso-motor, action of the splanchnics.

We must now attempt to give a brief *résumé* of the recent experimental researches that have been undertaken in cerebral physiology and pathology.

The view very generally entertained in regard to the hemispheres of the brain, up to quite a recent period, was that it was impossible to excite them by any direct stimulus. Longet even went so far as to say,—as subsequent experiments have shown very erroneously, that galvanic currents might be passed in every direction without any evidence of involuntary or voluntary muscular contractility being perceptible. Flourens, Matteucci, Weber, Budge, V. Deen, and Schiff, all came to the same conclusion. It is very difficult to assign any reasons for this singular unanimity in error amongst really well-practised physiological observers, though it may, perhaps, be partly attributable to the employment of too much violence in exposure of the brain and want of care in preventing loss of blood, both of which readily lead to exhaustion of the brain and incapacity to respond to irritation; partly to the use of far too violent stimuli, and partly to parts of the brain having been accidentally irritated that subsequent experiments have shown to be more excitable. Hitzig states that he was led to undertake the series of experiments with which his name is now connected by the discovery that, on transmitting constant galvanic currents through the posterior region of the head, movements of the eyes could readily be produced. He then tried an experiment or two upon the rabbit, and, finding that he succeeded in obtaining movements, he at once associated himself with Herr Fritsch and performed a series of experiments, first upon non-narcotized and afterwards upon narcotized animals (dogs). The source of electricity in these experiments was 10 Daniell's cells with an induction apparatus and the exposure of the brain was effected by means of a trephine. The two experimenters soon arrived at the following conclusions:

A part of the convexity of the cerebrum of the dog is motor, another part is non-motor. The motor part lies, speaking broadly, more towards the anterior, the non-motor more towards the posterior part. By means of electrical irritation of the motor part coordinated muscular movements of the opposite side of the body can be obtained. These muscular contractions, when feeble currents are employed, may affect quite localised and limited groups of

muscles. With stronger currents other muscles are brought into action, and with still stronger ones the muscles on the corresponding side of the body.

The centre for the muscles of the neck was demonstrated by their experiments to lie in the lateral portion of the præfrontal gyrus. The extreme outer part of the post-frontal gyrus conceals the centre for the extensors and adductors of the fore leg. A little posterior to this, and nearer the coronal fissure, are the centres for the flexion and rotation of the fore-limbs. The facialis is innervated from the middle part of the supra-Sylvian fissure. They were unable to discover any precise spot of irritation, to which contraction of the back, belly, or tail muscles responded with certainty. Tetanizing currents were not constant in their effects. E. Weber, it appears, had many years before noticed, though the fact had fallen into oblivion, that after opening a tetanizing current which had been transmitted through the spinal cord of a frog, secondary movements occurred in all the muscles of the body. Hitzig found that a very similar phenomenon occurs after tetanizing the cerebral substance even when the current has only been transmitted for a few seconds altogether; secondary movements appearing on breaking the current, which assume a trembling character in the facial region, whilst they present more of the character of clonic spasms in the extremities. In two instances well-marked attacks of epilepsy followed the passage of the currents.

They made also the interesting observation that, as loss of blood proceeds, the excitability of the brain sinks with great rapidity and disappears entirely, even when the strongest currents were used, as soon as death took place. The dura mater they found to be sensitive immediately after exposure, and its sensitiveness to rapidly increase. The pia mater is destitute of sensibility. The general results of their observations in this their first conjoint work led them to the conclusion that the mind or soul (Seele) was by no means, as Flourens and many other physiologists had maintained, a kind of compound function of the entire mass of the cerebrum, the expression of which could be abolished in its entirety, but not partially, by mechanical means; but that it was rather to be regarded as a collection of several intellectual functions originating from circumscribed centres in the cortex of the cerebrum. These observations and results were published in 'Reichert und Dubois Reymond's Archiv' for 1870, heft 3.

In a fresh series of researches undertaken to elucidate the physiology of the cerebrum, and published in 'Reichert und Dubois Reymond's Archiv' for 1873 (heft 3 and 4), Hitzig studied the effects of various strengths and directions of the

current, and of various toxic agents in modifying these effects. He found in the first place that the anode always produced a more powerful effect than the cathode. Then that in animals so deeply ætherized that all trace of reflex action was abolished the electrical excitability of the brain was partly preserved, partly destroyed. Further, that morphia could be administered even in large doses, both internally and by hypodermic injection, without reaction ceasing on the application of the stimulus. The same was found to hold good for apnœa, for in whatever method this was induced absence of the electric excitability of the cerebrum was never observed.

Hitzig, in his last work, after giving briefly the results of his investigations as already detailed, proceeds to make a vigorous attack on Prof. Ferrier, accusing him of having appropriated his results without sufficient acknowledgment, especially in regard to the production of epilepsy from tetanizing the brain, and to the effects of loss of blood in lowering or abolishing the excitability of the cerebrum, and charging him with having neglected the literature of the subject. He likewise takes exception to the strength of the current used, to the position in which the poles were placed, and winds up with a fiery denunciation of his whole proceedings.

Nothnagel's experiments were chiefly made on rabbits by the injection of minute quantities of chromic acid into selected regions of the brain, but he has also employed the method of puncture and destruction of certain limited portions with a view of ascertaining their function. He finds that injection of chromic acid into the posterior part of the cerebrum, or even puncture with a fine needle, produces the most violent spasmodic movements, which last for a minute or two. No alteration in the sensibility of any part of the body was subsequently observable; no disturbance resulted from irritation of the cornu ammonis. Injection of the chromic acid into the upper surface of the hemisphere, a little to one side of the median line and about half an inch or a little more from the anterior margin, caused extension and abduction of the fore limb and to some extent also of the hind limb, which endured for several days and then gradually passed off. He attributes this result to partial paralysis of the muscular sense. Irritation of the nucleus caudatus of the corpus striatum caused the animal to hop forward, then to rest, and then to move forward again on slight irritation, till at length, the intervals becoming shorter and shorter, it rushed forward at high speed. It at length fell over, but the legs continued to move. Death took place in twenty-four hours.

Nothnagel[1] has also cut away the two nuclei lenticulares or

[1] 'Centralblatt,' 1873, p. 882.

corpora striata externa,—the deepest basal portion, *i.e.* the loop of the basis cruris, however being retained, and finds that the animal is exactly in the condition of one deprived of the cerebral hemisphere, all spontaneous movement ceasing, and the animal sitting quiescent, if not irritated, till death occurs. On the other hand the reflex movements are very lively. In this operation the nucleus caudatus may remain quite intact. In a subsequent series of experiments[1] he removed or destroyed the *thalami optici*, and found that these centres have no influence over the voluntary movements, nor after their removal is any change perceptible in the sensibility of the skin. They appear rather to be connected with the muscular sense.

Fournié, of whose researches an abstract is given by Dr. Sterling in the 'Journal of Anatomy and Physiology' (vol. viii, 1874, p. 179), gives the details of some experiments the results of which are not quite in accord with those obtained by Nothnagel. He used chloride of zinc as the destructive agent, and found that complete destruction of the thalamus opticus caused complete abolition of the sense of touch; that lesion of the corpus striatum constantly produced paralysis of motion; that lesion of the cornu ammonis was followed by the same phenomena as lesion of the other convolutions, the animal losing also the sense of equilibrium. The results are sufficiently striking, and at the same time sufficiently different from those of Nothnagel to show that a large amount of experiment and observation is required before any general conclusions can be drawn.

In a paper read before the Royal Society[2] Dr. Sanderson related experiments he had made with a view of testing the accuracy of Fritsch and Hitzig's observations, and which led him to maintain the view formerly generally entertained by physiologists, to the effect that the function of co-ordinating voluntary movements is not localised in the convolutions, but lower down in the cerebro-spinal centres; for he found that, in certain cases in which the superficial convolutions containing centres were removed, while in others the whole of the anterior part of the left hemisphere as far down as the outer portion of the corpus striatum was taken away with the aid of a sharpened spoon, if the cut surface of the white substance were stimulated by induced currents, movements of the opposite side of the body are produced which are of the same character as those that result from excitation of the natural surface; 2, that the excitability is limited to certain spots, which can be as sharply defined as those demonstrable on the natural surface; and 3, that the relative position of the active spots on the cut and natural surface respectively correspond with each other.

[1] 'Centralblatt,' 1874, p. 882.

[2] Abstract in 'Nature,' 1874, p. 245.

Dr. Ferrier, pursuing the same line of investigation as that commenced by Hitzig, has also studied the effects of electrical excitation of the various parts of the brain. The current he employed was obtained from one small Stöhrer's cell, with carbon and zinc elements, and the induced current of the secondary coil of Dubois Reymond's magneto-electrometer. As a rule, the current was not stronger than could be borne without great discomfort on the tip of the tongue. Dr. Ferrier found, as Hitzig had done, that the excitability of the brain is greatly depressed, and recommends that special regions should be selected for examination; the bone covering these may be removed by a trephine or other convenient instrument without doing more damage to other parts than can be avoided. He too finds that the dura mater is extremely sensitive, the application of the electrodes to the brain substance causing functional hyperæmia, and often leading to profuse hæmorrhage from the sinuses which in the quiescent state of the brain had ceased to bleed. This phenomenon, he points out, is quite in accordance with the general fact that every organ in the active performance of its normal function receives a large supply of blood, whilst it also gives material support to the view that in the effects observed we are witnessing only the ordinary action of the parts when excited. It is a curious circumstance that the brain of fowls is insusceptible of electric stimulation, and Dr. Ferrier associates it with the fact discovered by Weir Mitchell that pigeons and fowls are not amenable to the influence of opium.

The remarkable uniformity and great definiteness of the results in the experiments performed by Dr. Ferrier, like those of Hitzig, show clearly that the phenomena presented were by no means chance effects of the electrical excitation distributed through the whole nervous mass, and acting now on this and now on that centre. To give an instance or two. In one observation the electrodes (the distance of the secondary coil being 6 cm.), being placed on the anterior or orbital extremity of the supra-Sylvian convolution of the brain of a cat, the animal, which had been lying torpid, suddenly started up, threw back its head, opened its eyes widely, lashed its tail, panted, screamed, and spit as if in furious rage. This observation was several times repeated, the animal at the end of each such manifestation sinking down into its stupid condition. In another experiment on the same animal the electrodes were applied to the posterior part of the superior external convolution, when it immediately exhibited signs of pain, screamed and kicked out with its left hind leg, at the same time turning its head and looking behind in an astonished manner. In other experiments particular muscles, as those of the ear, eyelid, fore paw, hind

leg, tail, &c., could be called into play by stimulation of particular regions of the cerebral surface with great precision, and this not only in different animals of the same species, but in animals of different species, as the cat, dog, and rabbit, the general accordance of the position of the centres being very remarkable. Comparative experiments proved further that the two sides of the brain are symmetrical.

The general results of Ferrier's experiments demonstrate—though it yet remains to be seen whether the observation and inquiry of others will corroborate his statements—that epileptiform fits can be produced by passing electric currents through the cerebral convolutions; the fact that they do not occur immediately on the application of the electrodes, but only when a certain interval has elapsed after their withdrawal, being regarded by Dr. Ferrier rightly as evidence that the grey matter has to reach, as Hughlings-Jackson has shown to be the case in ordinary epileptic fits, a certain pitch of tension before an explosive discharge takes place. In the next place he has shown that electric stimulation of the convolution which in cats and dogs runs on either side of the median fissure, and which is termed the superior external convolution, both in front of and behind the crucial sulcus, contains the centres governing the movements of the extremities; those effecting rotation of head to opposite side (in part), retraction of opposite ear and elevation of nostril and corner of mouth of opposite side with closure of opposite eye, are situated in the middle external convolution, whilst those for the rotation of the head (in part) and affecting the movements of the tongue and jaws are situated in the inferior external convolution.

When the exhaustion of the animal had proceeded to such an extent that the convolutions no longer responded to electric stimulation, the corpus callosum was divided and the ganglia at the base of the cerebrum were excited. The application of the electrodes to the corpus striatum in dogs caused rigid pleurosthotonos, the action being always crossed. Removal of the electrodes caused entire cessation of the spasm. Shifting of the electrodes to the optic thalamus on either side produced no effect, nor was any visible effect produced by irritation of the hippocampus or of the fornix. Irritation of the *corpora quadrigemina* gave very definite results, for in rabbits the application of the electrodes to the anterior tubercles caused immediate and violent opisthotonic spasm and extension of the extremities, the pupils were widely dilated and the jaws were firmly clenched, and they would hence appear to be (though they probably have other functions) centres for the exterior muscles. Irritation of the *cerebellum* produced comparatively little effect. The eyeballs were,

however, affected with violent nystagmus and some other minor movements of the face, and each separate lobule (in rabbits) is or appears to be a distinct centre for special alterations of the optic axis. On the integrity of these centres depends the maintenance of the equilibrium of the body.

The question of the relation between the convolutions as motor centres and the parts of the cerebral hemispheres which are subservient to the mental processes more immediately related to these definite muscular movements, Dr. Ferrier answers by referring to cases of aphasia, and showing that in these cases the ideational centres are situate in the same regions as the corresponding motor centres. We have no space to follow Dr. Ferrier into the pathological relations of his paper, but must leave our readers to peruse them at leisure in the very interesting volumes in which they are contained.

The object of M. Luys' work is to show that amongst the phenomena of cerebral activity there are a considerable number which may be regarded as of a reflex nature, resembling in this respect the operations taking place in the spinal cord and being executed in an automatic manner, entirely apart from the domain of will and conscious mind. In pursuing this line of inquiry, M. Luys first enters into a brief discussion of the principal characters of purely reflex spinal actions and the laws that preside over their mechanism and evolution, and then endeavours to show the points of similarity presented to them by reflex cerebral actions, both in regard to the constitution of the organic substratum in which they are manifested and in regard to the general dynamic conditions under which they are exhibited.

M. Luys then proceeds to consider in succession the mode of dispersion of sensory impressions in the plexuses of the cortical substance—the anatomical constitution of the *sensorium commune*—the general laws that preside over the transformation of purely sensory changes into purely psychical changes—the mode in which sensory impressions are reflected as motor acts and finally, how in all consciously or unconsciously performed motor acts there is a more or less latent *satellite* sensorial impression which constitutes a prime mover of the subsequent *processus*. He then goes on to discuss the pathological evidence bearing upon the subject.

In reference, then, first, to reflex spinal acts, these may be regarded as motor reactions resulting from the excitation of the proper elements of the spinal cord. The substratum essential to these manifestations is the grey substance of the posterior and anterior regions of the cord, the excitation of groups of cells in the former producing changes which result in the discharge of

force acting on corresponding and *satellite* groups in the latter, whence it is distributed by the anterior roots of the spinal nerves to the muscles. Every reflex manifestation is capable of being decomposed into three parts—1, a period of incidence; 2, an intermediate period, during which the nervous elements of the cord are excited to activity, and 3, a period of remission or conduction through centrifugal conductors; and thus it may be said broadly, that every reflex action is a secondary phenomenon which under no circumstances originates by itself.

On comparing reflex cerebral actions with reflex spinal actions many points of similarity are at once seen to exist. In the first place, the substratum of the processes here represented by the cortical substance of the brain is composed of numerous layers of cells so intimately connected together that it is easy to understand that they may vibrate in unison. These cells vary in size, the most superficial being small, whilst those more deeply seated are large, thus exhibiting a certain analogy to the small cells of the posterior cornua, and the large ones of the anterior cornua of the spinal cord, with which anatomical peculiarities Luys thinks functional attributes may go hand in hand. He therefore holds that impressions received, say on the retina, are directly transmissible to the submeningeal layers of the cortical substance, and are thence reflected upon the larger and deeper cells, whence impulses are directed to the sensorium. "It is in consequence of this intimate action and reaction between the incident impression and the central elements that the transformed, purified, and so to speak spiritualised impression—which nevertheless preserves its original character—gives origin to *psychical* excitation properly so called." . . . . "The psychical disturbance, once set up in the plexuses of the *sensorium*, continues its progressive evolution, and may reappear in the form of co-ordinate motor excitations. This view, it must be acknowledged, derives considerable support from the recent experiments of Hitzig, Ferrier, and others, who have shown the existence of localised centres in the cerebral convolutions. The conclusions that Luys draws from the analogy between the ordinary reflex spinal and reflex cerebral acts are of rather a startling character. "Voluntary movement," he goes on to say, "which we are accustomed to regard as the best example of spontaneity and as the most direct expression of human personality, is, looking at its genesis and mode of evolution, a subaltern phenomenon, an action *en retour* linked to a sensory impression, to an antecedent emotion, to some disturbance of the sensorium; in a word, so far as regards its intimate mechanism, its mode of extrinsic projection, it is only a reflex action more or less conscious, more or less automatic, enlarged

and rendered perfect by the play of the various elements which enter into its constitution," and just as special motor regions of the spinal cord may be called into action under the influence of various excitations which are not their usual stimuli, so the motor centres in the brain present the same variations and the same docility in obeying excitations which are not habitual to them. Thus, under normal conditions the central motor zones, which preside over the emission of articulated sounds, respond to acoustic impressions; but under certain conditions these same motor zones cease to obey their habitual stimulus, and by an artifice of education become capable of receiving excitation from another sensorial source. Thus, in the action of reading in a loud voice visual impressions are substituted for audition in order to produce articulation. In the action of writing from dictation visual and auditory impressions unite to direct the movements of the fingers. In the blind who in a loud voice read the pages of a book specially prepared for their use tactile replace the visual impressions of those who see.

He then proceeds to consider the characters differentiating reflex cerebral from reflex spinal actions, which, as far as we can understand them, are essentially due to the consequences of the radiation of impressions in the plexuses of the sensorium, which radiation causes a spiritualisation of the impression. These he considers in the order of period of incidence, period of propagation, and period of emission, but this part of M. Luys' work we must frankly confess is beyond our depth, and seems to us somewhat imaginative. In regard to the period of incidence, however, he notices some interesting facts showing the persistence of impressions, owing to the continuance of the vibrations of the sensorial apparatus, both central and peripheral. Thus, those who have worked much at the microscope know that after several hours' steady application, the images of the objects studied can, on closing the eyes, be made to reappear with all their objective characters. So after a long journey the sound of the wheels of the carriage is still heard, and Dr. Moos, of Heidelberg, has known an instance where particular sounds continued to be heard for fifteen days, and in another case, in which the patient was a professor of music, sounds were reproduced for several hours after each lesson. This remarkable property, he remarks, that the nervous elements possess of retaining for a period of variable duration the impressions that have disturbed (les ont ébranlés) them is one of the best marked points in the genesis and regular evolution of cerebral reflex action. It is owing to this that when they have once occurred they are apt to be renewed and perpetuated with the same rhythm and the same character that they possessed when conceived for the first

time; and so whilst *reflex spinal actions* only act to the extent that an appeal is made to the grey substance by the excitation of a posterior root, and in order to obtain a new discharge a fresh peripheric stimulus must be applied, *reflex cerebral actions*, on the contrary, once developed, repeat themselves *motu proprio* by the special properties of the nervous elements which execute them, and by the active participation of peripheric as well as of central regions. The *incident* impression is not wholly extinguished when it has produced its effect, but it plants itself in the nervous substance and, like an imported seed, germinates, perpetuates itself, expands itself, and again incessantly undergoes revivification like a stored-up *vis viva*, which nourishes itself and only awaits an appropriate moment to enter into action.

In regard to the period of propagation the cerebral differ from the spinal reflex actions in the superaddition of the activity of certain zones of the cortical substance in which the active manifestations of the intellect occur, and which are subjacent to those of the sensorium, and M. Luys enters minutely into the proportion in which the activity of these zones is superadded in reflex cerebral actions and the period of the evolution of the *processus* at which the activity is superadded.

Lastly, in the period of emission cerebral reflex actions are characterised by their becoming enlarged and amplified by the activity of new zones of the cortical substance which are successively brought into play—in fact, they are rendered more complex.

In the next section M. Luys considers the reflex manifestations of psychical activity, which he again divides into the three periods, mentioned above, of incidence, propagation, and reflexion. Here as elsewhere the commencement of the *processus* is marked by phenomena of sensibility, and its completion by phenomena of movement.

The phenomena of imitation spring physiologically from the fundamental property of the nervous elements, disposed or directed in a certain way by an external impression to place themselves automatically in unison with this impression, just as a tuning-fork is thrown into vibration if a second fork of the same pitch is set into vibration near it. The imitative accommodation of the elements of the sensorium results from the intervention of two series of external impressions, acoustic and visual. It is owing to the former that speech becomes implanted in the sensorium of the young infant, and that by placing its larynx in unison with the sounds that it hears—it reproduces the sounds heard by imitation. It is owing to the visual impressions that man learns to write, which is only the imitation of the graphic sounds presented to his eyes, that

he acquires manual dexterity in the arts and learns drawing, that he models his movements. In morbid conditions imitation may lead to epidemic.

The reflex manifestation of the intellectual operations obey the same general laws. They have for their regions of emergence the zones of intellectual activity, and appear to have in the substance of the cortical layers, as pathological evidence also shows, isolated masses of cells specially distinct for their evolution. The act of speech presents in its physiological evolution the highest expression of cerebral activity. The *processes* of which it is composed are divisible into a period of incidence, which corresponds to the arrival of the acoustic impression and its diffusion in those regions of the cortical substance destined to receive it; into a period of propagation, during which the vibration reaches the purely intellectual regions and leads to the participation of the conscious individual; and into a period of reflexion, in which the same primordial vibration, after propagation through the whole extent of the cortical plexus and after having excited the activity of its elements, is discharged as a synthesis of multiple activities, and passes into the region of automatic activity, which translates it into precise articulated sounds.

From this partial condensation of M. Luys' work our readers will be able to judge of the line of argument he adopts and the general scope of the memoir. We cannot say it is wanting in precision, but at the same time, it does no more than place in a clear light what has been frequently advanced before, namely, that the intellectual operations, even the highest, are very closely allied to ordinary reflex acts.

VI.—Reports on Poor Law Relief.[1]

The population of England and Wales was estimated at 23,000,000 and 23,300,000 respectively for the years 1872 and 1873. The total amount expended in relief to the poor in the same years was 8,007,403 and 7,692,169 pounds. The amount expended in relief to the poor per head of the estimated population was 6s. 11½d. and

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[1] 1. *Annual Report of the Local Government for Ireland, being the Second report under "The Local Government Board (Ireland) Act,"* 35 *and* 36 *Vic., c.* 69 ; *with Appendices.* 1874.

2. *Second Annual Report of the Local Government Board.*

3. *Third Annual Report of the Local Government Board.*

4. *Report of the Executive Committee of the Dublin Sanitary Association to the General Meeting, held July* 2, 1874, *&c.*

6s. 7¼d. for each year respectively. The rate in the pound expended for poor relief on the annual value of ratable property in 1872 was 1s. 5·6d; in the same year the net annual value of ratable property was £109,447,111. The indoor paupers decreased from 156,795 in 1872 to 154,171 in 1873, and the outdoor paupers from 824,247 in 1872 to 736,201 in 1873. The class of adult able-bodied persons (including paupers receiving relief on account of the sickness of themselves or their families, and a large number of widows) decreased from 25,035 indoor to 22,053, and from 128,718 outdoor to 105,644 in the course of the same period.

Turning to Ireland, with a population of 5,411,000 according to the census of 1871, it appears that in 1873 the expenditure was £959,736, or 1s. 5¼d. in the pound, for all purposes, viz. relief, medical relief, burial grounds, registration of births, deaths, and marriages, and sanitary measures. For poor relief in Ireland the expenditure amounted in 1873 to £790,560, being at the rate of 1s. 2¼d. on the net annual valuation, which was £13,408,226 ; and its cost per head, in relation to the population, was 2s. 11¾d. On the other hand, it is computed that the poor relief amounts to 6s. 11½d. per head in England and to 5s. 2¼d. in Scotland. As seen above the poor rate in Ireland covers also medical relief, charges for burial grounds, for registration of births, deaths, and marriages, and for for sanitary measures. This comparatively low cost for poor relief in Ireland has been attributed to the beneficial working of the system of medical relief in operation for many years, whereby medical care is extended to persons not strictly classed as paupers, on the principle that sickness is one of the chief factors of pauperism ; but this great saving is not attained without hardship on certain classes. The dissipated, the spendthrift, and the improvident, who avail themselves of relief at the public cost, press hardly on the industrious working classes. So likewise, whilst the medical profession contributes its full share of the public expenditure, its members are called upon, at most inadequate salaries, to undertake the treatment of all applicants for dispensary relief, although, as is well known, orders for such relief are given very loosely and indiscriminately, and fall into the hands of well-to-do artisans and small tradesmen, who justly ought to pay themselves for the medical attendance they require, and thereby afford remunerative practice to the medical men.

Indeed, if we look to the figures given in the report of the Irish Local Government Board, expressing the amount of relief afforded at the public cost, we are almost dismayed at its rapid increase, and the more so in face of the fact of decreasing population. For instance, the expenditure on outdoor relief has advanced by rapid strides from £29,748 in 1866 to £91,154 in 1873; whilst within the same period the population has diminished from 5,582,625, to 5,344,151, a reduction of no less than 238,474 souls. So, again, we are in-

formed that in the course of the twenty years during which the Irish dispensaries have been administered by boards of guardians, under Government control, the annual number of cases receiving gratuitous medical aid have increased by 2000, notwithstanding a reduction of the population within the same period of very nearly a million. Further, the issue of tickets for home attendance increased from 193,784 in 1871 to 210,859 in 1872.

The resort by a population to state relief in the rapidly progressive ratio indicated is in itself an evil, and points to defects in administration. It also affords evidence of a sad want of self-dependence on the part of such a population, of the degradation of its *morale,* of an evil and mischievous tendency to become hangers-on upon public aid to gratify a morbid desire to save the cost of assistance it can and should afford to meet. Much of the abuse and misuse of such relief is attributable to the agency in whose hands its administration is lodged. Small tradespeople, publicans, ex-policemen, and such-like persons, particularly in small towns, cannot be expected to be discreet distributors of the relief they are entrusted to bestow. The trust confided to them is in their hands an act of patronage and consequently open to grave abuse. The mode of appointment of guardians, circumstances connected with the appointment, the fact that honorary and other of the guardians of a higher order of intelligence and of superior social position hold aloof from the meetings and the business of the board—these and other conditions detract from the efficiency of the appointed controllers of state medical relief by leaving the latter in the hands of those, on the one hand, less qualified to administer it equitably and judiciously, and, on the other, more open to improper influences and possibly selfish motives.

The increasing abuse of state medical relief, and the detection of several instances of imposition—one of which is known as the "system of preparation" and resorted to constantly before applying for a medical certificate—have aroused the attention of some boards of guardians. The following extract from the Brixworth Union Committee is to the point:—"Perhaps [it is written] no part of the system of out-door relief is more open to abuse than medical relief, or requires more care and supervision on the part of the board. A very large proportion of the cases now on the out-door relief list obtained relief in the first instance through medical certificates for some illness, either feigned or only of a temporary nature, and have now become reduced to a state of almost hopeless pauperism. Your committee would here notice the remark of one of the medical officers belonging to the union, that he is sorry to confess that there is a very strong tendency on the part of the poor to feign disease, and would particularly call the attention of the board to a complaint of the same medical officer, that it is not at all an unfrequent thing for his report on a case to be set aside or over-ruled by the applicant for relief procuring an out-patient's letter to the infirmary from some chari-

table person, on the strength of which he applies again and obtains out-door relief."

Another fact worthy of notice, brought out in the last Irish Report and in the one preceding, is, the remarkable discrepancy found, in the several provinces, in respect to the number of relief tickets granted by the individuals authorised to give them (we may add given by unauthorised persons also) and subsequently cancelled by the committees of management, on the ground of improper distribution, the individuals obtaining them being found not entitled to gratuitous medical relief. For instance, during the last year the committees in Ulster cancelled 270 tickets, in Munster 97, in Leinster 103, and in Connaught 79.

Assuming what is probably true, that the committees in Ulster exercise the power of cancelling tickets without any undue harshness, and that the persons who issue tickets are as careful as in the other provinces not to bestow them on improper objects, and bearing in mind also the number of tickets issued respectively in the four provinces, viz. for last year, 167,905 in Ulster, 213,392 in Munster, 228,782 in Leinster, and 81,947 in Connaught, there ought to have been cancelled in that year in Munster and Leinster, instead of 97 and 103 tickets, considerably more than 270 in each, and in Connaught about half that number instead of 79. The obvious intention of the legislature in confiding to the committees the power of cancelling dispensary relief tickets issued by the individuals privileged to grant them was, to prevent the abuse of the privilege, in the event of such relief being bestowed on persons whose circumstances would admit of their procuring medical attendance for themselves; and the figures quoted above certainly justify the suspicion that the committees in Leinster, Munster, and Connaught, do not exercise the power of cancelling dispensary relief tickets with judicious strictness.

The cases mentioned and the statements put forward in the medical and general journals of the abuse of the funds allocated to the relief of the poor call for inquiry; indeed, we cannot but repeat the opinion lately given by gentlemen unconnected with this branch of the public service, at large and influential meetings at the Colleges of Physicians and Surgeons, Ireland, namely, that boards of guardians are not as a general rule suited for the administration of medical relief, and that such an important service should be placed wholly under Government control. We would certainly advise that responsible and paid officers should alone issue tickets for poor law medical relief, that they should be obliged to pay themselves or obtain from the recipient of the medical relief a suitable fee or fees in all cases where it is ascertained the persons so relieved were not in need of gratuitous aid. The custom of filling the posts of relieving officer and of subsanitary officer with policemen or retired policemen that still prevails but too generally in Ireland, notwithstanding the Government decision

against the custom in England, is attended with most injurious consequences and should not be allowed.

We drew attention some years since to the value of the system of poor relief pursued in parts of Germany, and are pleased to see this system brought under notice in the last Report of the English Local Government Board. Boards of guardians, both in England and Ireland, ought to acquaint themselves with the information thus given, and they would likewise receive much enlightenment and instruction by the perusal in the Second English Report of the remarks on out-door relief, including the many able communications put on record in the appendix.

In a previous page we have referred to the inadequacy of the remuneration of dispensary medical officers under the auspices of the poor law guardians, and we are sorry to add that the same enlightened functionaries exhibit no higher appreciation of the services of medical men whom they call upon to officiate as sanitary medical officers under the recent Public Health Act for Ireland (1874). The largess of a five-pound note, and in some instances the appointment of the honour of the thing, is all the reward allotted to many medical men for the sanitary supervision of as many as forty-seven or more square miles of country, and of several thousands of people. Things may not be so bad in England, but even there the nobly sounding admonition to the authorised sanitary bodies to appoint really competent persons to discharge the important duties of sanitary medical officers has been to little purpose, and equally fruitless has been the advice tendered that "the Board think it right to point out that this object [the securing of thoroughly efficient officers] will not be attained unless the salary which the authorities are able to offer is such as to afford an adequate remuneration for the services required."

The importance of quarantine, when guided by scientific and reasonable regulations, is daily becoming more evident and something has been done in Ireland for "*providing against the introduction of* cholera, *and for the treatment of* cholera *patients* arriving in any vessel."[1]

The last Report of the Local Government Board for Ireland notes that port nuisance authorities now have jurisdiction over vessels afloat, not only in regard to cholera, but to every other dangerous contagious disease, such as small pox, typhus, scarlatina, &c., and that this power relates not only to vessels from foreign ports, but also to arrivals from England, Scotland, &c., in coasting vessels, such as colliers, &c.

Until the last session difficulty in carrying out the provisions of the Quarantine Act was experienced on account of the existence of

[1] In our notice of "Irish Poor Law Medical Relief," Jan., 1874, attention was drawn to the necessity for the regular supervision and isolation of arrivals from infected ports.

various nuisance authorities, as in the case of the Cove of Cork, a harbour, perhaps, the most exposed to the importation of disease of any in Ireland. A short act was accordingly sought for and obtained, by which one of the nuisance authorities was empowered to deal more effectually with the matter, and the other neighbouring authorities were made contributory to the expenditure. This power was extended to the whole coast by the Local Government Board, which has now obtained the former powers of the Privy Council in Ireland in connection with sanitary affairs.

In every case the Local Government Board has made a board of guardians responsible for the charge of the port nuisance district. We cannot extol the wisdom of this proceeding, as we think boards of guardians are not suited, in the great majority of cases, for the exercise of such powers by the attainments and enlightenment of their members generally. In exemplification of the general unfitness of boards of guardians for the sanitary functions allotted them by Acts of Parliament, we may refer to the records of a recent meeting, at which the chairman, oblivious of the sad history of the introduction of smallpox by the ships and passengers into Dublin, urged, in his sapient opposition to the maintenance of a hospital for arrivals of sick at the port within the jurisdiction of himself and colleagues, that "he never knew of disease being propagated by the arrival of a sick person in port."

In some places intercepting hospitals have been established; some of these, however, are only small vessels moored in the ports.

Much difficulty has been found in the endeavour to obtain suitable buildings or sites for the erection of hospitals owing to the disinclination of Governmental or other public departments, or of individuals, to part with property for such purposes. Hence the Local Government Board consider that a case has been made out for applying the compulsory power of purchasing land for the sites of hospitals for dangerous contagious diseases, as well as for the establishment of new cemeteries.

In Ireland, by the recent Act of Parliament, a riparian nuisance authority, though made contributory to expenses incurred by the port nuisance authority in regard to ships afloat, retains all its duties and powers as a nuisance authority ashore, which is not the case with port nuisance authorities established under the English Public Health Act, 1871.

Great credit is due to those ladies and gentlemen, foremost among whom is Miss Octavia Hill, who are taking in hand the systematic visiting of the poor and destitute in London, particularly with the view of teaching them how best to improve and elevate themselves. We confess we long to see like efforts made systematically and more generally in Dublin and throughout Ireland; and whilst we gladly bear testimony to the heroic efforts now being made there, as well as to what has been done in times past, we must say that we are w

aware of the peculiar difficulties that beset the path of the genuine philanthropist in Ireland.

At this moment many places in Dublin and in other parts of Ireland can boast of schools with considerable numbers of young persons taught and housed, who were a short time since roaming as true types of the street arab. The same good has been effected in regard to others who were constantly arriving in Dublin and other chief towns to run the risk of a career of vice, by means of many useful charities conducted by private resources and private individuals. In this way many of the poor are rendered useful members of the community and many hardy lads are being reared and yearly enter the public services afloat or ashore.

Sanitary associations for the purpose of educating public opinion upon the enormous gain to the nation and the community to be obtained from the efficient working of applied sanitary science have taken root at Dublin, and in two or three of the chief provincial towns of Ireland; from such sources of information we trust even guardians may learn something.

The population of Ireland is far more rural than is the case in England, the (registered) mortality accordingly was less as regards the whole island in the following proportions respectively during the first and second quarters of the present year (1874) than in England, viz. 19·5 and 18·2 to 23·4 and 21·0 per 1000.

The birth-rate, on the contrary, was but 27·9 and 29·0, to 36·8 and 36·9 per 1000 of that in England.

The emigration from Ireland[1] was 1·56 and that from England ·56 per cent. of the respective populations in 1873. From these three points of view therefore we come to understand the steady diminution of the Irish population and the increase of that of England, notwithstanding the somewhat superior healthfulness of Ireland. But this latter appears hardly sustainable, to any extent at least, inasmuch as the early months of infant life add greatly to the death-rate, and as infants are over one fourth less numerous proportionally. In Ireland much of the apparently diminished death-rate may thus be explained away. It is not clear to us but that if an actuary worked out these figures in their entirety the apparent healthfulness of Ireland would entirely disappear, and the result would be altogether to exhibit the actual Irish mortality as decidedly heavier than that of England. One of the effects of emigration appears to be to produce a serious weakening of the population; according to the returns of the Registrar-General of the estimated population of Ireland to the middle of 1874 the females outnumber the males by no less than 173,371, a very serious number in the small aggregate population which now numbers only 5,300,485.

Before dismissing the important subject of emigration in reference

[1] 'Report on Emigration from the United Kingdom,' 1873, Marine Department, Board of Trade.

to our subject, we may observe that the number of female emigrants from Ireland during the year 1873 was in the proportion of 24,648 to 28,553 males, whilst the male and female emigrants from England in the same year were relatively to each other as 140,715 to 92,170.

It is therefore a sound policy to follow up the encouragement given by some energetic philanthropists to female emigration from the old countries.

The serious lesson taught by the smallpox epidemic of 1871, 1872 and 1873, is written, so that those who run may read, in the Irish Report. The deaths from that disease in 1864 were 854, and 347, 187, 20, 19, 20, 32, 647, 3197, and 481, in each succeeding year respectively. During the two first quarters of the present year it is sad to observe the mortality from this loathsome disease has been 141 and 123 respectively. The Registrar-General's report adds that of these deaths 130 occurred in Ulster, 10 in Leinster, and 1 in Munster, whilst Connaught escaped altogether. The mortality in Ulster is attributed to the baneful practice of inoculation, still practised in the wilds of Donegal despite the vigilance of the authorities, and to imported cases from Scotland. The Registrar of the Dunkineely district, Donegal Union, advises that, besides the operator, the parents of the person inoculated should be made legally responsible for the inoculation of the child.

It is to be hoped that energetic measures will be taken, possibly in the direction indicated by the Registrar of Dunkineely, to stop so criminal a practice. To this remark we regret to have to add that the fatality of zymotic disease in Ireland, and especially in the large towns, points to the necessity for the exercise of great vigilance and all the powers and machinery now placed by the legislature in the hands of the authorities to lessen the frequency and severity of all classes of preventable disease.

The appointments already made in Ireland under the Public Health Act 1874, have shown that relationship to guardians of influence in their respective unions, and other considerations apart from special fitness for the office, have operated as motives, in some cases, in determining the choice made. Indeed, it is curious to observe the number of offices of analyst and consulting sanitary medical officer that have been conferred on one or two individuals who by some means or other stand in favour with poor law guardians and other sanitary authorities.

We do not know whether the approval of the Local Government Board will be accorded to these appointments, but it is certain that already a strong feeling is becoming manifest, not only among the profession, but also among the general public, in reference to this matter, and we trust that an appeal will be made to the legislature if a correction of this growing abuse be not speedily effected.

In the first six months of the present year no less than 1641 persons died of fever in Ireland and 1491 of scarlatina.

Such figures show the wisdom of Parliament in passing the Health Act for Ireland this year, and it remains for us to hope that sufficient encouragement will be given, by those in authority, by sufficient remuneration and by continuous support in every way to the medical officers, on whom the real weight and responsibility of effectively carrying out the law will fall.

The heavy sickness and mortality now prevalent in the larger cities and towns in Ireland points to a very defective state of public health.

On the other hand, there is undoubtedly an increasing spirit of commercial enterprise, of self-dependence and inquiry, among the energetic and intelligent classes in Ireland, as evidenced in the growing disposition of active men of business and others to examine for themselves the exact state of public health, taxation, and sanitary matters, great and small. These exertions and less of political clap-trap will, we trust, bear good fruit and tend to create a more healthful, contented, and industrious people than has been the case in not a few instances throughout the Green Isle in times past.

VII.—Dental Pathology and Surgery.[1]

A RECENT review gave us occasion to remark that few branches of the surgical profession were making more comparative progress than that of dental surgery, and it is now our duty to criticise a book the high character of which seems to us to be worthy of a few special remarks, for which we would crave our readers' patience, as a prologue to our task. We have always maintained that the speciality of which we speak should, under certain conditions, be looked upon, of right, as a branch of surgery; of course we speak of it in its highest and most extended view, for, when so considered, it will be seen that no practitioner can do full justice to those who come under his care unless he possesses an adequate knowledge of the principles of medicine and of surgery. These views are confirmed by a distinguished surgeon, Mr. Savory, who, in the course of an excellent address[2] given by him on the subject of dental education, whilst urging his hearers to take advantage of the highest general professional study, begs them "to thoroughly realise the fact that the teeth are parts of the living body and hold important relations to various other parts; that they are not isolated structures in their diseases and morbid actions," and "that, in a word, although they have, as every other part has, a special, they have not, nevertheless, an isolated pathology, and this because, although they have, as every other part has, a special, they have not, nevertheless, an isolated anatomy and physiology."

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1 *Dental Pathology and Surgery.* By S. JAMES SALTER, M.B., F.R.S. 1874.
2 An 'Address on Dental Education,' by W. S. Savory, F.R.S. 1874.

In America the medical papers have been indulging in an exciting discussion as to the status of those who practise dentistry[1] in relation to the medical profession, refusing to grant an equal position to dental practitioners, and we think with every reason, *unless* they have a *medical* qualification.

Two of the great objections to all professional examinations in America are these:—Firstly, no preliminary tests of a liberal general education are required. Secondly, most schools, whether special or general, confer their own diplomas. In England all is different. Recognised institutions alone can grant degrees, and the consequence of this most healthy system is that, socially, with many brilliant exceptions across the Atlantic, a higher type of man is found in the ranks of our profession. We shall ever support the view that every so-called "dentist" should hold at least a surgical degree, which is for the most part the case with all the more distinguished members of that specialty, though it is not as well known as it should be, even amongst professional men, that a special degree is granted by the Royal College of Surgeons, which is known by the appellation of Licentiate in Dental Surgery, or by the letters L.D.S., this degree implying such an education in a general hospital in the subjects of medicine, surgery, anatomy, &c., as may be gained during a two years' course of study in any of the London and in some of the provincial hospitals, this being additional to a special curriculum which also extends over a period of two years. Such a diploma, emanating from a body so distinguished as that constituting the College of Surgeons, cannot but be of great value, though we look forward to the time when the degree of L.D.S. like that of L.M., a collateral degree, may only be taken as accessory to the membership or fellowship of the College, this insuring in addition to other benefits the incalculable advantages of a preliminary test in the subjects of a liberal education. However, *Chi va piano va sano e lontano.* We cannot expect everything to be done at once; great advances have been made in a brief period, and we trust that ere long every dentist will be a fully qualified practitioner, so that he may rightly be termed a dental surgeon.

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[1] Looking at the case of those who by insufficient general education are only qualified to practise the specialty in a purely mechanical point of view.

. . . . The most eminent men in America are working to bring about such a standard of education as exists in England, and we think it due to them to quote an expression of opinion which has come to our notice since this review was written, by Dr. Garretson, who has not only the reputation of being one of the first operating surgeons in America, but is also a distinguished dental practitioner. In a recent article in the 'Philadelphia Medical Times,' he says—"A mere specialist cannot, and should not, be received into the common brotherhood of medicine without his coming to a common platform as regards all that makes the doctor. The ophthalmic surgeon has recognised this; and to-day he holds just such a position as it is desirable he should enjoy—he has his special hospitals, but he has no college, no peculiar diploma.

"His general education is *alike* with his medical brethren, and therefore his privileges are the *same.*"

As a member of the staff of a large metropolitan hospital we have been much struck with the ignorance of even advanced students exhibited in relation to diseases of the teeth, some acquaintance with which is of no slight importance to every surgeon, for very serious operations have been undertaken in connection with the jaws where a modicum of special knowledge might have rendered comparatively trivial measures quite as successful.

It is a great error to think that even the lesser operations undertaken by the dental surgeon are by any means simple. They have to be performed under considerable difficulties. Light and perfect freedom from moisture are necessities, both being difficult to obtain, whilst manipulations of the most delicate character have to be undertaken in very small cavities, placed again within an area—the oral cavity—itself of no great dimensions.

It must also be remembered that in many cases it is the province of the dental surgeon to see the first signs of diseases which by timely interference could be shorn of their chief terrors; witness the case of certain tumours of the maxillæ, the incipient signs of amaurosis when the result of dental lesion, and epithelioma of the tongue which has been too often mistaken by the patient for a simple ulcer. It is not long since we saw a case of this disease which had been treated by a medically unqualified practitioner as a simple abrasion by the potent remedy of tincture of myrrh and water, much to the patient's satisfaction, until the disease was so far advanced and the glands so much involved that all operative interference was quite out of the question. The confiding patient had been thus treated for two years, with no desire to seek further advice.

Whilst insisting upon the importance of a knowledge of medicine and surgery to the dental practitioner, we would conversely impress upon his medical confrère the advantage of some knowledge of dental diseases, for thereby hints may be obtained for treatment which might not otherwise present themselves. Cases will be given to prove this proposition in the course of this article.

At present the chief means of education in dental surgery is at a special hospital, in which the teaching is as good as any to be obtained, but, for the reasons we have stated, we look forward to the time when every general hospital will have a *special* department for the instruction of students in all that concerns diseases of the teeth, which instruction shall be received by the College of Surgeons as fulfilling the necessities of the curriculum required by it. We do not for one moment wish to reflect upon any well-conducted special institutions as long as their only object is help to the needy and instruction to the learner apart from all cliqueism and self-interest on the part of those conducting them. Many of them have been undoubtedly of the greatest service in the treatment of disease, but we think that, as a general principle, every large hospital should do

all in its power to give instruction within its walls in *every* branch of medicine and surgery. In saying this we by no means speak of dental surgery alone, though it gives us pleasure to state that its importance has been fully acknowledged, and that two hospitals in London have already instituted special departments in this branch, lectures being given in every subject connected therewith.

The book which we have undertaken to review seems to be the best apology for these preliminary remarks, inasmuch as it treats of dental surgery in its highest branches—a subject treated with much skill and acumen by one most fully able in every point of view to uphold the claims of this speciality in surgery.

Mr. Salter commences with a résumé of the general anatomy and physiology of the teeth, his description of their tissues being only given in a brief manner, so as to show their bearings upon and relation to dental disease. This brevity, however, is characterised by peculiar conciseness and lucidity, a measure of praise which cannot be rendered to far more ambitious expositions of one of the most difficult subjects within the range of minute anatomy.

Mr. Salter maintains that enamel is totally devoid of vitality, and that it undergoes no vital or nutritional changes, and that any alteration which may take place in it is the result of chemical and physical agencies. In this view we coincide, but would suggest that there is often some congenital defect of tissue which points out the way to morbid action.

A much more extended consideration is given to the anatomical and physiological characteristics of dentine, and the relation of its peculiar histology to its vital manifestations is entered into critically at length. This question is of great scientific and practical interest, and the literature connected with it is not a little curious. Writers on the anatomy of the teeth up to a comparatively recent time looked upon dentine in the light of a non-sentient structure. This was conspicuously the case with Hunter. The closer observations, however, of practitioners of dental surgery, even many years ago, showed that ivory is really endowed with sensation, and according to the author this was first pointed out by M. Duval[1] in a paper read before the Academy of Medicine of Paris in 1831. Next occurred the interesting question, how is the sentient perception of dentinal tissue established? This including two other problems to be unravelled, viz., the connection of the ivory with the nervous system and the locality and character of the nerves supplying it.

The nerves of the tooth pulp had been seen to end in broad loops, near the surface of that organ and had never been further traced. The question remained thus unanswered until the year 1856, when

[1] 'Observations pratiques sur la Sensibilité des Substances dures des Dents.' J. R. Duval. Paris, 1833.

Mr. Tomes thought that he had given a solution to it in the course of discoveries which he published in the 'Philosophical Transactions of the Royal Society' of that year, in which he declared that the tubuli of the dentine were occupied by "soft fibrils," which he designated "organs of sensation," and these he figured as displayed in decalcified dentine and also as emanating from the surface of the pulp.

Until that time all physiologists had attributed to the dentinal tubes the same circulatory and nutritional functions as the analogous spaces in bone. Lacunæ and canaliculi, and their identity of office, had been assumed to be more evident from the fact that the tubes of the dentine in the fangs of the teeth often open into the substance of the bone surrounding them. Mr. Salter assumes that Mr. Tomes mistook the soft fibrils for *nerves,* but this is a charge which we cannot hold as proven, though he undoubtedly looked upon them as a means of imparting sensibility to the dentine, as proved by his terming them fibrils and organs of sensation. Should, however, Mr. Salter be right in his conjecture as to Mr. Tomes's meaning and that the cylindrical processes he figured were nerves, then dentine would be more largely supplied with sentient nerves than any other tissue of the body, except, perhaps, one or more of the organs of special sense, for it must be recollected that dentine, bare and uncovered in many of the lower animals, and sometimes in man, is used for crushing the hardest foods without pain or even discomfort, and that therefore it seems too much to believe that a sixth or at least an eighth of its bulk is composed of sentient nerves. On these points Mr. Salter expresses his views most clearly, maintaining that the cylindrical tubes which Mr Tomes in 1856 described as "organs of sensation" are nothing more than the dentinal tubes themselves, and he thinks that that gentleman's view had its origin in the optical illusion, by which a "broad brilliant ring of considerable thickness" seems to surround the very thin wall of the dentinal tubes when seen in a transverse section. The matter is thus summed up : "These minute tubules of dentine, when free and soft, have a certain resemblance to nerve fibres, and this circumstance, combined with the belief that the luminous rings around the tubes represent the wall of comparatively large hollow canals, has led a distinguished microscopist to conclude that they are nerves, or the equivalents of nerves, occupying the cavities of such canals." Mr. Salter then claims that the histological facts had been recorded "both in description and illustration" by that great father of human histology, Henle, some fifteen years before, (1841),[1] whilst at the same time he draws attention to Lent's discoveries concerning the tubes in 1854.[2] We have been at some pains in investigating this interesting controversy and

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[1] Vide Tomes's 'Manual of Dental Surgery,' 3rd edition, 1874.

[2] Vide Sommering's 'Baue des Menslichen Körpers.' Leipsig, 1841.

have compared Henle's figure (Plate V, fig. 11)[1] with the original one of Mr. Tomes in the 'Philosophical Transactions,' and in this case think that Mr. Salter has partly proved his assertion; but we must also, in justice to both parties, draw his attention to the fact that with regard to the contents of the tubes, of which we shall speak anon, Mr. Tomes has almost entirely changed or modified his views since he wrote in 1856, whilst Mr. Salter has seemed to base his arguments in this and other instances upon earlier editions of that gentleman's works. Something has been recently done to decide the source of sensation in dentine. A German microscopist, Franz Boll, has found that from the extreme nerve loops in the tooth pulp large numbers of very minute fibrils proceed outwards, passing between the odontoblasts and their tubular prolongations.[2] Speaking of this important discovery, the author remarks, "It is highly probable that these are the nervous elements distributed to the dentine, but whether they pass into the inter-tubular substance or fastening upon the tube walls are so piloted into the ivory structure is quite uncertain. It is, however, unlikely that they pierce the walls of the ivory cell and occupy the axis of the tube."

With regard to the contents of the tubes, Mr. Salter thinks that they are occupied by a dense albuminous plasma, and he founds his opinion upon the results of much individual observation. This being so it is entitled to every consideration, though we ourselves are more in favour of the views of that well-known microscopist, Dr. Lionel Beale, who looks upon the tubular contents as "bioplasm," or "germinal matter," and as such intimately connected with the formation of dentine. We are somewhat surprised that his theories have not been alluded to in this connection, but this is doubtless through an oversight. Mr. Salter also mentions another physiological fact of interest, viz. that their contents are liable to be stained yellow in jaundice and red or purple, owing to decomposed hæmatine, when the pulp is disorganized.

We are pleased to see that Mr. Salter holds strongly the vitality of dentine, a doctrine which is in his judgment proved by the fact that the teeth maintain through life a permanent register of alternate growth, complete and incomplete. He here alludes to the indications of this growth, the laminated structure of the teeth indicating the increments of their development, and to these indications he proposes to give a name which seems to us to be well worth repeating, viz., "incremental lines," this being surely far better and significant than the "contour lines" of Prof. Owen, which designation really does not give a true idea of the formation of the structure. The one difficulty in most of the views presented with regard to the sentient character of dentine is the fact that its

[1] French edition.

[2] 'Siebold und Kolliker's Zeitschrift,' 1854, p. 121.

sensibility is in great part, though not always wholly, destroyed by death of the central pulp. Mr. Salter would ascribe this second sensitiveness, if it may so be termed, to some nervous relation with the periosteum, this is a by no means improbable hypothesis, but, of course, most difficult of practical proof.

Next follows a very interesting and original chapter upon the mechanism of articulation. The general view that the vowels (*vocales*) are true voice sounds and that consonants (*con sono*) are merely methods of commencing and terminating vowels is opposed; this notion having arisen from the supposition that voice sounds are necessary for intelligible utterance, whereas most articulate sounds may be pronounced as sibilants without any vocalisation whatever, as any person who may attempt to pronounce V or S may soon discover, thus it seems that the vowel sounds depend upon the relations existing between the size of the oral cavity and the size of its orifices, consonantal sounds depending on some "superadded condition of orifice quite independent of size." For example in *far* and *fear*, the difference is in the sounding of the vowels, the condition of the oral cavity and apertures being most capacious in *far;* but in two terminals like *oth* and *oss* the sound is consonantal, the modification in sound being produced by no change in size of the cavity but by the changed relations of the tongue to the lingual surfaces of the teeth, the teeth acting as a kind of sounding board, as it were, against which the tongue assumes the action of a valve, whereby closure, modified or complete, is effected. The seat of sound is not where the tongue touches the teeth, but in that spot where the contact is not perfect. The importance of this and of further explanations which space does not permit us to consider cannot be overestimated in attempts to restore imperfect articulation through loss of teeth. Indeed, this chapter is of interest to all who have pleasure in studying the intricate mechanism of speech. Whilst considering the subject of supernumerary teeth, the alleged occurrence of third sets is considered, a question which has been the subject of as many doubts as Mr. Thoms has applied to the existence of centenarians. Mr. Salter proves that Hunter never really asserted that he had actually seen an example of third dentition, merely saying that he had seen a case in which "two fore teeth shot up in the lower jaw," which he thought might be members of a third set. Doubtless these were either supernumerary or impacted teeth which had not made their way to the surface of the maxilla until late in life, their tardy appearance probably being owing to absorption of the alveolar process after the loss of the teeth. Further allusion will be made to these impacted teeth when showing their important relation to many tumours connected with the maxillary bones. Some instructive cases are also quoted in which dental deficiency has occurred in conjunction with general alopœcia,

thus proving the relations of the teeth to the dermal system. No novel views are promulgated with regard to the subject of dental irregularities, but we strongly concur in the view as to the necessity of heroic measures in such cases, being convinced that the timely removal of certain teeth, whether bicuspids or molars, is one of the most potent means of remedying such deformities—considerable experience of the results of practice opposed to this, especially amongst American practitioners, having served to confirm such treatment in every particular. In a chapter on united teeth a new classification is adopted, these being divided into, first, dentinal or congenital; second, cemental or secondary; and in another important chapter on secondary dentine the author would divide it into three sub-classes, viz., *a*, dentine of repair; *b*, dentine excrescence; *c*, osteo-dentine; the distinction between the first and the last variety being that in the one case it is the result of lesion of the primary dentine, either by abrasion or carious action, and is always found in connection with the true dentine, whilst in the other it is always found towards the extremity of the fang and is quite detached from the ordinary dentine.

With regard to the dentine of repair it may be mentioned that the dentinal tubuli are not unfrequently filled with a secondary deposit which renders the structure clear and translucent as horn. This question of reparation of tissue, for such we believe it to be, is most instructive in its bearing upon that *quæstio vexata* concerning the vitality and sensibility of dentine. To ourselves this formation of secondary dentine within the tubules seems to settle the point in favour of the champions of vitality, although we cannot quite coincide with the author's view that dentine of repair is *always* the result of "lesion of the dentine of the primary system."

It now remains to consider these phenomena in relation to decay of dental tissue. Some maintain that caries is altogether the result of external causes, and quite independent of any vital action whatever; and they attempt to support this view by the assertion that carious action takes place in the case of bone or dead teeth placed in contact with the oral secretions as substitutes for living teeth, but to any other than the superficial observer the microscopical appearances which characterise true and false caries are very dissimilar. In the latter the area of decay is unlimited, and there is an absence of that calcified zone which is an attempt of nature to arrest or form a boundary to the incursion of disease. Moreover, Mr. Salter mentions the entire absence of the characteristic carious smell. We consider that the author and M. Magitot have quite maintained their position in considering caries a vital process as far as the ossification of the tubes is concerned. The same thing is seen in the crown of teeth worn by attrition, the object being to form

a protective covering for the pulp. The same changes sometimes occur in the fangs of the teeth of young children, the whole process being one of vital nutrition. Indubitably certain external causes, such as depraved secretions, the decomposition of food, &c. are factors at least in the production of disease; but at the same time imperfect tissue nutrition has much more to do with the induction of disease. Indeed, after three years' experiment, we have seen so much success in arresting a tendency to caries in young children, by constitutional remedies, that that alone would commend these views strongly to our favour; and were we to seek further proofs that the teeth are not as it were so many pegs or nails fixed into the cranium, it is only necessary to refer to the changes occurring in the teeth as a result of illness, pregnancy, or change of climate. We are convinced that the less the dental surgeon upholds the contracted views of a pure specialist the more he will devote his attention to the constitutional treatment of disease. The subject of the mechanical treatment of decay is not entered upon as pertaining rather, as Mr. Salter expresses it, to "dentistry" proper than to the sphere of "dental surgery."

In a chapter on Odontomes, viz. tumours connected with the hard tissues of the teeth, the author proposes a new classification, which he considers preferable to that of M. Broca, who first conferred the happy name of odontome upon a class of tumours which, before the advent of his valuable treatise, had been but little understood, this type of tumour being produced by hypertrophy and abnormal development of the dental tissues occurring during the embryonic condition of the teeth. M. Broca divided odontomes into four principal classes, these deriving their generic name from the period of their formation.

Firstly, Odontomes embryoplastiques; those which occur before the definite development of the dental tissues commences. Secondly, Odontomes odontoplastiques; those which occur after the odontoblast cells have assumed their function in the formation of tissue. Thirdly, Odontomes coronaires; those connected with the period during which the coronal tissues are formed. Fourthly, Odontomes radiculaires; those which are connected with the formation of the root.

Wedl, we believe, objects to this arrangement as inconvenient and physiologically inaccurate; and Mr. Salter, having a like opinion, would divide these tumours into two classes.

Firstly, congenital Odontomes; including under this classification warty teeth, hernia of the fang, enamel nodules on fangs; and, secondly, secondary or induced Odontomes—these including exostosis, and dentine excrescence.

However accurate may be this nomenclature, it seems to represent a rather too great multiplication of terms; and, moreover, the

method and kind of development of the various classes of Odontome is not so clearly expressed as by that of Broca.

Several of these tumours have been described; but the most remarkable on record is one operated upon by M. Forget, in the person of the son of a banker, of Guadaloupe. The disease occurred in the inferior maxilla, and seemed to commence at about five years of age, the first symptoms being characterised by intermittent pain, which subsequently became permanent and excruciating. At the age of twenty the tumour had attained the size of a large egg; the alveolar ridge contained *no teeth save the first bicuspid;* whilst at the base of the tumour were many fistulous openings. The operation undertaken for its relief was the ordinary one of *resection*, which, of course, involved much deformity; the result, however, was permanent and satisfactory. The mass, when examined, was found to contain the two molars imperfectly developed, the tumour representing a heterogeneous conglomeration of osseous tissue, dentine crusta petrosa. Doubtless a union of the germs of the aforesaid molars, induced by inflammatory action, was primarily the *fons et origo mali,* this leading ultimately to abnormal development of bony tissue. We may here incidentally remark that this tumour was originally described by M. Forget, and by Mr. Tomes as an exostosis; whilst Mr. Salter pronounced that it probably consisted of a dilatation of all the tissues of the fang. This was also his opinion with regard to a specimen in Mr. Tomes' possession, which Mr. Charles Tomes has since most clearly described, proving that it was not an exostosis after all, although his views differ slightly from those of the author. These tumours are always benign, and are invariably *encysted*—a point never to be lost sight of; for the character of the tumour being determined, there is no necessity for such an operation as resection, which in too many cases leaves sad deformity behind, the loss of bone and of the antagonising power of the muscles on the two sides of the face often leading to total deprivation of masticating power. In these cases much simpler measures will suffice; for the removal of a little superficial bone will often permit of the encysted mass being *shelled out,* without intervention of lion forceps and saw. The diagnosis of these tumours may be effected with but little difficulty. It must be remembered that an Odontome is an abnormal growth having its point of departure and development in irregularity of dental evolution. Starting from this hypothesis, we find that a contracted maxilla, or an overcrowded arch, may cause evolutional irregularity, by giving no room to certain teeth, the embryos of which are in the jaw. Therefore, whenever a tumour is found in connection with the absence of certain teeth in the alveolar ridge, it may be assumed that it holds a relation to the absent teeth. This chapter is concluded with many interesting descriptions of examples of tumours in

the author's possession, by which he illustrates the newly proposed classification quoted above.

In the consideration of diseases of the gums, mention is made of a disease which is but rarely seen, and has not been described before —this is termed "Transparent hypertrophy of the gum." The morbid change consists in the slow but progressive hypertrophy of the extreme edges of the gum, this ridge being sharply defined in relation to the neighbouring healthy structure. The parts affected are of a pale pink colour and semi-transparent and of almost cartilaginous consistence. As a result of this condition the teeth become slowly dislocated and the alveolar processes absorbed.

In connection with the subject of impaction of the permanent teeth within the substance of the jaw many curious examples are given, these showing how their presence may be the origin of many obscure or not easily explained diseases, such as neuralgia, cystic disease, &c. Mr. Salter described one very remarkable specimen of dentigerous cyst, which is certainly unique.[1] Ordinarily the tooth contained in a cyst of this character is a permanent one, but in this case it is a supernumerary tooth which lies free and unconnected in a very delicate capsule of bone about the size of a walnut, almost round in shape and starting from the base of the antrum, though quite unattached to its lateral walls. The expansion is not that of the antral parietes at all but of the bony loculus of the contained tooth. Mr. Salter refers to this valuable specimen as being of the greatest anatomical interest in those serous enlargements of the antrum connected with the presence of inverted teeth. These cases are often described as expansions of the antrum itself, and it is argued that there is no reason why a tooth being "cut" through the walls of the antral cavity, should produce such a result, inasmuch as he says that the crown of an inverted tooth in the nose produces no irritation or increased secretion. On this point we are not in a position to offer corroboration or the reverse, but the author has seen three cases of such a description, and founding his opinion upon such observation he promulgates the theory that most dentigerous tumours of the kind under consideration have commenced as cysts in the bone at the basis of the maxillary sinus, expanding into it, occluding its cavity and dilating its walls. If this be so, the true walls of the cyst would be the ossified tooth capsule, that is the enamel pulp, between which and the tooth the fluid is secreted. Next painful eruption of the dentes sapientiæ is treated of, and mention is made of mistakes in diagnosis where the painful cutting of these teeth has been taken for scrofulous caries of bone, syphilis, and even cancer. We need not remind our readers of the instructive brochure written by M. Nélaton on a similar subject.

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[1] In the possession of Mr. Samuel Cartwright.

The next chapter is one pregnant with interest alike to physician, surgeon, and specialist, being devoted to affections of the nervous system dependent upon diseases of the teeth.

It is a well known fact that a large proportion of the neuralgiæ are associated with pain in the head and neck, and that none of the ordinary theories on neuralgia, acute and subtle though they be, serve to assist the practitioner much in his treatment of such cases, indeed, we do not think that we are asserting too much when we affirm that, as a rule, these affections—so often the bane of a whole life time—are treated empirically: nervine tonics are made use of, then some of the vaunted specifics for the disease, arsenic, phosphorus, *cum multis aliis*. The cases which yield to treatment are for the most part those which are manifestly connected with constitutional mischief: indigestion, anæmia, and the like. Appropriate treatment in the one case or the other relieving pain, but even in such successful cases the primary nerve lesion is cured we know not how.

Again, in those cases where neuralgia is connected with diseases engendered by malarial influence, quinine or arsenic will often effect a cure, but here again, we are ignorant of the action of the remedy employed, that is to say, as far as the disease is concerned. The same obtains in many other examples.

The late Dr. Anstie maintained that true neuralgic pain held its court, so to speak, in the posterior root of the spinal nerve in which pain was experienced, and that the tissue of this root always exhibited more or less atrophy. This pathological condition is not unfrequently found, but still it does not satisfactorily explain the *immediate* removal of long lasting pain when the cause is discovered and removed, *e. g.* neuralgia relieved instantaneously by the removal of an impacted tooth or of one in which the pulp has been exposed or by the dissecting out of a cicatrix in which a filament of nerve has been involved. We cannot but think that the question of reflex pain in connection with dental and other causes of peripheral excitation has not been adequately considered, and inasmuch as many of the most severe and intractable examples of neuralgic pain are connected with the teeth we think that Mr. Salter has earned our gratitude for directing especial attention to them.

It is not sufficient for the physician, when he has exhausted his pharmacopœia in attempts to cure disease, to tell the sufferer "to go to his dentist," but he should also discover whether he is fully qualified by his professional education to aid him in his research for the original cause of nervous excitation. It is not within our province at this time to consider neuralgic pain, save in its relation to lesions connected with the upper extremity. The affections of the nervous system dependent upon the teeth may be divided into two classes, viz., those which are reflex—secondary and remote—and those which are direct. In the former category would be placed

epilepsy, neuralgia, and paralysis, whilst in the latter would occur—local pain, facial palsy, &c. Pain most often occurs as the result of reflex nervous irritation in the supra and infra orbital nerves, the globe of the eye and the temple, and Mr. Salter especially mentions a spot near the vertex of the skull, "a little on one side—the side of the affected tooth," whilst he observes that the integument at the painful spot becomes "hot, tender, and red." The trigeminus is the nerve most obnoxious to reflex action and, when its branches are affected, pain is frequently referred to branches of the cervical and brachial plexuses. The author draws attention to the fact first observed by Dr. Anstie, that wounds of the ulnar nerve cause reflex neuralgia of the fifth pair. The converse may occur and we have at present a most interesting case under our observation in which the existence of such communication is fully proved. The patient we allude to has suffered agonies for several years from intermittent neuralgia, the pains generally occurring about once a week—these extend over the whole of the head, the neck, and the shoulder, but the pain always commences at one particular spot over the malar bone. Within a few hours of the commencement of the attack an erruption which is almost pustular in character (and here the proved connection between neuralgia and some cases of shingles must not be forgotten) seems to delineate as on a map the course of the affected nerves, whilst synchronously with the pain in the cheek she has the most acute pain in the ulnar nerve at the elbow joint "just as if you had pinched the funny bone very hard," as she describes it graphically.

The disease does not seem to be at all connected with the teeth. The cutaneous phenomena and the periodicity of the attacks seemed to indicate the use of arsenic, which treatment has been so successful that the attack now occurs at intervals of three weeks instead of one week, and each one seems to become slighter in character. It should be mentioned that the patient is healthy, with all her functions regular, and that preliminary to this treatment all teeth which could possibly have been a source of trouble were removed with no benefit whatever. Trousseau, in his clinical lectures suggested that in neuralgia not associated with special lesion, there is generally great sensitiveness when pressure is made upon the seventh cervical vertebra, a symptom most remarkably apparent in this case where the patient without a symptom of hysteria, had not a hint as to the object of the examination along the course of the spinal cord.

Mr. Salter mentions the following diseases connected with the teeth as most frequently causing exalted nervous irritability—"Caries, with or without exposure, Exostosis, Hypertrophy of the crusta petrosa, Nodular developments of dentine in the pulp cavity. Periostitis—plastic or suppurative. Impaction of permanent teeth in maxillary bones. Crowding of teeth from insufficient room."

All these have a special mode of diagnosis with which it would be a great advantage for all practitioners to be acquainted, and we must here express a regret that, with the accurate pathology the author has given of all these diseases and with his peculiar powers of illustration, he has not entered more fully into the description of their diagnostic symptoms. The chapter concludes with several narrative cases, some of which are so unique that they will bear repetition. One is given in the words of the late Dr. Hyde Salter, the author's brother, of which, however, space will only permit a *résumé*. Dr. Salter had a carious tooth, a lower left anterior molar, which had not given him any trouble for many years—he had suffered for some time from acute neuralgic pain, its situation being "in the neck, clavicular and supra mammary region" being exactly the position "of the descending cutaneous branches of the cervical plexus and the part in the arm where the aching was the most intense and intolerable was the insertion of the deltoid." The suspected tooth had no pain in it whatever nor was there any sign of inflammation in the adjacent gum, but Dr. Salter had an intuitive feeling that it was the source of his trouble and therefore had it extracted. From the moment of the operation the symptoms ceased and there was never the slightest return of them.

This is a comparatively simple case, though had Dr. Salter been content with the knowledge that no tooth gave him pain he might have lost much time in resorting to remedies which would never have been permanently successful. In every case of neuralgia questions should not only be asked, but every tooth should be separately inspected, and the whole economy thoroughly examined for any lesion which may exist, and by the irritation of which it is the cause, may destroy the finely adjusted balance of the nervous system. One of the most fertile fields for such discoveries is the mouth for the most decided cases of neuralgia are undoubtedly dental in origin. Other mischief may ensue from these lesions. Prof. Galenzowski relates a case[1] of amaurosis produced primarily by the impaction of a portion of toothpick by the side of a molar tooth. The tooth was removed when the foreign body was discovered at the end of its fang. As a result of the removal all pain ceased, the same evening the eye became sensible to light, and nine days afterwards F. P. could see as well with the left eye as with the right, after a blindness in it of thirteen months.

Amongst cases of neuralgia caused by exostosis on the fangs of teeth a remarkable example is recorded. Miss B. P. had been from early childhood a patient of Mr. Bell: during her first dentition nothing had occurred to attract attention, but during adolescence, she was attacked with pains of a neuralgic type affecting the

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[1] 'Archives Générales de Médecine.'

maxillary nerves and their terminal branches. Mr. Bell's diagnosis was to the effect that these attacks were associated with certain teeth which gave some abnormal signs of sensibility. The disease rapidly progressed, the pain being soon no longer confined to the trigeminus but spreading "to the arms, the legs and indeed to the whole body." The upper front teeth were those suspected though to all outward appearance they were perfectly sound but when they were tapped lightly with an instrument, they gave slight evidence of pain. The suffering was now so intense that Mr. Bell removed the teeth exhibiting the above mentioned symptom and a close examination showed that although there was not an evidence of caries the fangs in each case were encrusted with *small nodules of exostosis.* A brief immunity from pain followed, but the same symptoms again recurred and concurrently other teeth became sensitive. "This succession of suffering, of tooth extraction, and of short lived ease was repeated again and again." Nodules of exostosis being discovered on the fangs of each tooth removed. At last when many teeth had been lost the lady's friends having some misgiving as to the propriety of the treatment sought advice from Sir B. Brodie, and the teeth, all of which had been preserved, were submitted to his inspection. Sir Benjamin deprecated the treatment, the patient persisted in approving it, and at her urgent request a further opinion was sought and she went to see Mr. Samuel Cartwright; the details of the case were related to him and the extracted teeth were put before him. After examining them with great care and attention he separated them into two lots, four bicuspids he placed on one side, declaring his belief that they were sound in every respect and could not have been associated with the painful affection which had been described to him. With regard to the remainder he considered that each and all of them might have caused the neuralgia, and he pointed out to the patient and her friends the nodules of exostosis on the fangs of the teeth as his reason for coming to that conclusion. Miss P. then explained to Mr. Cartwright that the four bicuspids which he had pronounced sound had been removed in early childhood for the regulation of her second dentition and that the others were all of them extracted "by Mr. Bell for her relief during her long and painful illness." With the removal of the last of the sensitive teeth her troubles ceased for ever.

Here the pain was caused by the pressure of the exostoses on the delicate nerve fibrils which ramify within the periosteal envelopments of the tooth; the same results may follow the formation of intrinsic dentine upon the parietes of the pulp cavity, substituting pulp for periosteum a similar explanation accounting for similar symptoms. It is needless to say what careful judgment must be exercised in such cases, for there is often no *apparent* lesion whatever in the teeth, and therefore much courage founded upon great confi-

dence is required in proposing the removal of the obnoxious organs. Unfortunately where either of these conditions exist, no therapeutic treatment will be of any save very transitory value. Another notable example is that of the wife of a medical man who, whenever a tooth on the left side of the inferior maxilla became affected with caries, however slightly, was immediately attacked with agonizing neuralgic pain at a small and circumscribed spot on the front of the left forearm about two inches above the bend of the elbow. Ultimately when she had lost several teeth and had a plate with artificial ones inserted, this irritated the mucous membrane at one part, and the same symptoms again manifested themselves and ceased at once on the removal of the cause of irritation.

A curious case of Amaurosis treated by Mr. Hancock is also mentioned. A boy came to the Charing Cross Hospital under these circumstances. About a month previously on waking one morning he found himself quite blind, having gone to bed enjoying perfect sight. The suddenness of the attack pointed to functional rather than structural mischief, and the teeth were examined. They were much crowded and wedged together. The overcrowding was relieved by the extraction of certain teeth, and the same evening the patient could distinguish light from darkness, the next morning he could distinguish objects, and he was soon dismissed cured.

Many other very interesting examples are mentioned, amongst them one of change in the colour of the iris, the result of prolonged neuralgia; also cases of deafness, paralysis, and epilepsy, all of which will well repay attention as examples of reflex disturbance dependent upon peripheral excitation of the nervous centres. The author also gives descriptions of direct affections caused by dental disease. We can but direct attention to these cases, for the limits of our space have been already transgressed, but we trust that we have shown sufficient to prove that the most severe cases of neuralgia may exist quite independently of any great degeneration of the nervous centres as supposed by some recent writers, for otherwise pain could not cease so suddenly with the removal of the cause of nervous excitation, and we believe that the teeth often escape blame and the physician loses a chance of successful treatment because there is no complaint of odontalgia, and the teeth remain unsuspected traitors. The causes are often quite inexplicable, but the practitioner should never desist from his efforts as long as there is the slightest hope of discovering the origin of the disease, whether it be simply some functional derangement, a hidden splinter, a morbid passion, a lurking parasite, or a diseased tooth. A discovery may be made at any moment leading to unexpected victory. Probably many of the cases which are benefited by

a course of iodide of potassium are connected with periosteal inflammation leading ultimately to thickening of bone or actual exostosis. Witness a case well known to all familiar with Sir Thomas Watson's lectures—that of Dr. Pemberton, who when on the high road to fame and fortune, was obliged to resign everything through tic douloureux. No remedy could alleviate his sufferings. Nerves were divided, portions of them excised, but all was done with no effect. When he died a post-mortem examination revealed the fact that the falciform process of the dura mater, near the crista galli, was partly ossified, this condition having doubtless been the cause of suffering. The opportunities of examining the pathology of neuralgia are few, but judging from knowledge obtained it is probable that in very many cases of pain about the head and neck there is an exostosed condition of the foramina through which the nerves pass, the pressure thus created being quite sufficient to explain every symptom.

Mr. Salter, in his observations on Phosphor Necrosis, does not add much to the articles written by him in Holmes' 'System of Surgery,' but he questions the correctness of Dr. Bristowe's views as expounded in his well-known monograph laid before the Privy Council in which he disputes the assertion that the local application of oxidised phosphorus to the exposed tooth pulp is essential to the induction of disease, basing his opinion upon the fact that some sufferers from the malady had assured him that they had never suffered from caries. Mr. Salter replies that had Dr. Bristowe been a dental surgeon and been acquainted with the ignorance of the poor as to the condition of their teeth he would not have considered their asseverations of the slightest value. He is confident, and we think with all reason, that in a large number of cases, in which a *superficial* examination might seem to corroborate such assertions, caries would still be found to exist, for even one experienced in such matters has difficulty in deciding upon the presence of certain examples of instertitial disease where the teeth are closely approximated together. The suggestion is made as a matter of speculation that it is not impossible that the phosphorus being accumulated by the periosteum may generate on the surface of the bone—a chemical superphosphate inconsistent with osteal vitality, the fact that this poison enters so largely into the composition of the skeleton rendering this hypothesis not altogether untenable. With regard to necrosis after the exanthemata and syphilitic periostitis the author thinks that the former is essentially the same in character as that resulting from the fumes of phosphorus, being the result of a local application of a specific poison to the vascular parts of the teeth which are undergoing great and rapid transitional stages at the age when the disease occurs. The book concludes with remarks on the extraction of teeth and the accidents

which may occur in connection therewith. The author here gives his own experiences most honestly and truthfully, many cases being mentioned which are against himself. No teaching is so valuable as that which represents the *litera scripta* of personal experience, especially when successful and unsuccessful results are all impartially noted together.

Especially is this chapter to be commended in a forensic point of view. Many casualties are quite unavoidable, yet the practitioner may be most unjustly condemned by an ignorant jury—especially if he be a man unknown to fame—for an accident which might have happened to the most skilful operator, and the author shows how the extraction of a tooth, simple as it appears to be, differs from other surgical operations. "In the one case," he says, "there is the keen cutting of sharp instruments where force is at a minimum in comparison with results obtained. In the other an organ has to be torn from its living connection with the rest of the body by force, and the operator is ignorant of the conditions with which he may have to deal," these depend upon the amount of adhesion between the tooth and the jaw, the divergence of its roots, an exostosed condition of their extremities, and the like.

In fine, he says, "unquestionably many teeth are broken in attempting to extract them which ought not to be broken, but in many other instances, *no amount of forethought or experience or mechanical dexterity* could have avoided a disaster, the circumstances producing which have been beyond the ken or the posssible prevention of the operator." Amongst the rare mishaps, he mentions the crushing of the inferior maxillary nerve, which may occur during the extraction of the dentes sapientiæ of the lower maxilla. When this happens there is instantaneous loss of sensation in all the teeth supplied by it, and the whole area of the lip and chin supplied by the mental nerve becomes absolutely numb. Sensation generally gradually returns. The book closes with a short chapter on artificial substitutes to be used in cases of cleft and perforate palate, and in which it is held that nothing is really so successful as operative procedure, an opinion in our judgment correct, for men like Fergusson and Pollock have so improved the operation that it is well nigh perfect. The *only* artificial substitute which really has any claims to great utility is that invented by an American surgeon, Dr. Norman Kingsley, of New York.

In summing up our criticism on this book we cannot but remark that the task of reviewing it has been throughout a pleasant one. The style is clear and perspicuous, the arrangement of the contents simple and accurate, the illustrations beyond reproach and in many examples marked by peculiar accuracy. Alexander is said to have asked Aristotle, "How he had made such great discoveries?" To which question the philosopher replied, "By facts, which never

lie." In this response may be found one of the chief charms and sources of strength in Mr. Salter's work. Its foundation is practical experience, and this is exemplified not only in a scientific point of view, for few have done more with the microscope in the elucidation of doubtful points in minute anatomy than the author, but equally in his descriptions of disease. To each one of which the word "clinical" would best apply. We regret that in some instances he has not entered more fully into the symptomatology of disease, for he has a ready method of explanation which we have never seen excelled in any book of this description. It will be found useful to both practitioner and student, and although it has some omissions, which we trust the author will supply in another volume; as, for instance, the treatment of fractures of the maxillæ, &c., we feel none the less inclined to give it highest praise, not only on account of its intrinsic merits, but because it is written with a single-minded and lofty purpose which is to show how much lies within the scope of dental surgery when its study is undertaken by those qualified to raise it, as he says, above "the narrow and circumscribed view" held by some specialists. He has well carried out his intention of showing "how serious are some of the maladies directly dependent on tooth disease, and how largely the pathology of the teeth is associated with serious morbid changes in contiguous structures." We confidently recommend this book to all our readers and would class it amongst those specimens of professional literature which confer lasting credit—the true "monumentum ære perennius" upon their authors.

VIII.—Pathology of Cerebral Hæmorrhage.[1]

(*Continued from vol.* liv. *p.* 107.)

Our knowledge of the mode of production of cerebral hæmorrhage is still most unsatisfactory, and the greatest variety of opinion exists as to the exact cause of it, says Dr. Maclagan, the translator of M. Bouchard's treatise. The difficulty of the question explains the divergence of opinions, remarks Jaccoud. It is not that there are greater difficulties in the study of cerebral hæmorrhage than of other diseases; but that, as every

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[1] 1. *A Study of some points in the Pathology of Cerebral Hæmorrhage.* By Ch. Bouchard, M.D. Translated by T. J. Maclagan, M.D.
2. *Cerebral Hæmorrhage and Apoplexy.* By J. Hughlings Jackson, M.D., F.R.C.P. ('Reynolds's System of Medicine,' vol. ii, 2nd edition.)
3. *A Treatise on Apoplexy, &c.* By John H. Lidell, A.M., M.D.
4. *Encéphale.—Hæmorrhagie Cerebrale.* Par Jaccoud et Hallopeau. ('Dictionnaire de Médecine, &c.,' tome xiii.)

one knows who has laboriously examined any pathological or other subject, sooner or later, the more carefully and minutely he pursues the investigation, the more difficult does the question appear to become. How often our opinions, formed from the seeming facts of yesterday, are negatived by the observations of to-day. So, too, whilst the most advanced researches bring fresh stores of knowledge, they do what is equally important—expose and define our ignorance; and this exposure, in its turn, ensures increasing knowledge; it bears a reproductive dissatisfaction.

If any disease be selected and its ætiology studied it is found that there are many agencies in operation, both without and within, seemingly capable of causing the morbid change. And it is exceedingly difficult—in fact, at present impossible—to determine how far one only has produced the disease, and what has been the influence of the remainder? If the clinical phenomena be next examined, it is also observed that different morbid changes, even in the same organ or organs, are not unfrequently manifested by very similar symptoms. Nor do we escape the difficulty in morbid anatomy; for though we see the effects of the morbid actions, we are made to learn that changes similar, but not precisely alike, are produced by different causes. And last of all, when knowledge of the causes of the actions and of the result has been gained, even then, not seeing the processes at work, we are unable to decide which of the several capable coexisting conditions produces the evil.

We are told by Dugald Stewart "that the ancients considered philosophy as the science of causes, and hence were led to many speculations to which the human faculties are altogether incompetent. They proposed to themselves to ascertain why a consequent follows from an antecedent, when in reality all that can be known is what consequents do follow." We confess, however, not knowing the extent of man's capability nor the limits of the possible, and observing the once considered impossible to be now in many instances possible, that we are more hopeful, if not confident, although we do not doubt that, whilst working on, we, as well as others, may many times have to experience the effect of hope deferred.

We are influenced and led by recognising the difficulties of our authors thus to give expression to the impediments ordinarily experienced; to have done so may not be altogether unprofitable. And we may usefully repeat that indefinite conclusions, defining, as they do, the lines of our ignorance, are sometimes even more serviceable than conclusions finite, which in physical science, at all events, must be to nature more or less unfaithful, thereby misleading, whilst to their authors seemingly true.

In this notice we do not purpose to do more than consider the leading features of the subject in question as set forth in the works before us, and finding that the authors of these works do not give any information beyond what is already known to the profession as to the causes acting from without the body, we shall, perhaps, gain more insight into the pathology of the disease if we examine, first the anatomical, and subsequently the clinical, observations they put before us.

It is now known that more or less hæmorrhage into brain may complicate many different diseases; not only scurvy, purpura, and malignant fevers, but glioma, abscess, red softening, and medullary cancer of brain. But the subject-matter of the treatise under consideration is not hæmorrhage into brain, but cases of cerebral hæmorrhage or so-called "sanguineous apoplexy."

Ample experience has shown that hæmorrhage in such cases occurs most frequently in the corpus striatum and optic thalamus, often in the pons, and very rarely in the cerebellum. The cerebrum in some cases is largely broken down by blood effusion, but the corpus striatum, optic thalamus, or both, are generally then also greatly disorganized; consequently it is thought that the bleeding most probably commenced in one or other of these ganglia.

In fatal cases, where the effusion is large, the blood frequently bursts into the lateral ventricle and makes its way into the third and fourth ventricles, and thus reaches the base. Dr. Jackson, however, tells us that he has twice known blood effused into the third ventricle without injury of the ganglia, but, as he says, these were exceptional cases. The blood in one of them came from a large aneurism seated in the middle line of the hinder part of the circle of Willis, at the divergence of the posterior cerebral arteries; in the other from an aneurism of a small artery of the posterior lobe.

The hæmorrhagic effusion is mostly single, but there may be two, three, or more extravasations. Dr. Jackson mentions a case of Dr. Baümler's in which there were four recent clots—a large one in the centre of the right hemisphere, a small one in each optic thalamus, and a small one in the right centre. Jaccoud and Hallopeau say that where there are several clots they are often found to occupy symmetrical parts in the two hemispheres, and that this arrangement accords with the distribution of the arterial lesions, for often aneurisms are found in the corresponding parts of each half of the encephalon. Moreover, all our authors concur in stating that not unfrequently the remains of older effusions are found in different parts of the brain; at times in the form of cysts, or of yellow

ochrey-looking changes, and sometimes, but not often, with forms of decided scar. Not many months ago we noticed in one of these old scars that the nerve-fibres were atrophied and surrounded by granules and crystals of hæmatoidin and a fatty fibroid material.

The size of the clot varies very much; it may be not larger than a pea, or as we, and probably others, have seen a large clot, may occupy almost the entire part of one hemisphere. Jaccoud and Hallopeau assert that, if the calibre of the vessel ruptured be very small and the tension therefore not great, the blood seems simply to separate the nerve-fibres, and a very small and elongated clot lies parallel with them. Oftener, however, it is found that the blood effused has ploughed up, so to speak, the brain substance, torn through the nerve-fibres and vessels, leaving a cavity the walls of which consist of shreddy lacerated nerve-fibres and torn vessels, the ends of which are plugged by minute blood-clots. Some of the extravasated blood is likewise infiltrated amongst the nerve-fibres to a greater or less distance beyond the cavity, and its sernm diffused a still greater distance beyond the blood-clot, and as a result the nerve elements are still more softened and disorganized.

Rochoux, Dr. Bouchard remarks, thought the resultant yellow-looking softening might be dependent on imbibition "where the extravasation lasted four or five days, but not when death occurred a few hours after the seizure." Dr. Bouchard, however, rightly observes that, although this softening may not be produced during the few hours which elapse between the occurrence of the hæmorrhage and death, yet it might be produced by imbibition after death—that is, in the interval between the cessation of life and the autopsy. And with a view to show that this softening is most probably the result of serous infiltration, he mentions the case of a woman who, while suffering from jaundice, was attacked more than once with cerebral hæmorrhage and subsequently died. All the tissues of her body were more or less coloured by bile, but this coloration was most marked in the substance of the brain around each of the apoplectic cavities to the depth of about half a centimètre. The cerebral tissue at these points was diminished in consistence, but presented no other histological changes. And he remarks, it would be difficult in this case not to attribute the additional coloration of the cerebral tissue around the cavities to the imbibition of the serum of the effused blood.

Jaccoud and Hallopeau also say the parts around the clot are in varying degree softened and infiltrated by the serum of the extravasated blood.

Dr. Bouchard records that he also found in the brain substance,

in the vicinity of the clot, granular bodies and aggregated fat-granules so called, these being seemingly the result of the disruptive necrobiotic changes. This morbid appearance, he says, may in some cases be traced from the seat of primary lesion even into the medulla.

Pathologists have usually attributed part of the softening also to inflammatory changes around the cavity; and Dr. Bouchard, too, holds that view, and mentions that in one case there was a remarkable multiplication of the nuclei of the connective tissue, and also of those of the capillary wall.

We may now inquire what changes were found in the vessels of the cases of cerebral hæmorrhage recorded by our authors. They all allude to fatty degeneration of the small arteries and capillaries, a change which has hitherto been regarded as a common, if not the most frequent, cause of cerebral hæmorrhage. As Dr. Bouchard reminds us, Paget, in 1850, first pointed out fatty change in the capillaries and small arteries of the brain and its connection with apoplexy. Robin, indeed, had in the previous year called attention to fatty degeneration of the arterial system, and of the small arteries of the brain especially, but he did not indicate that it was a cause of hæmorrhage. And from that time pathologists generally have considered this fatty degeneration to be a common cause of cerebral hæmorrhage. Likewise Wedl, in describing fatty degeneration of vessels, remarks in his work on 'Pathological Histology,' published in 1853, "It can scarcely be doubted that this degeneration of the walls of the vessels bears a causal relation to the subsequent hæmorrhage, as has been shown by Kölliker to be the case in the degenerated minute cerebral vessels in apoplexy."

We need hardly remind our readers that, about this time, much difference of opinion arose as to whether the fat-granules were outside the vessel or in its wall. By degrees, however, it was generally recognised that vessels lying amidst softened brain, hæmorrhagic or inflammatory, were frequently coated with or even embedded in fat-granules, and it was concluded that in such cases this vascular fatty change was evidently secondary; seemingly consequent on, and not the cause of the brain disorganization. Dr. Bouchard thinks it is now not difficult to explain how this discrepancy arose, and speaking of this he remarks, "M. Robin's discovery of the perivascular sheath of the small arteries of the brain enables us, by modifying the interpretation of Paget and Bennett,—the originators of these respective views,—to show how, by accurate observation of identical facts, they arrived at such opposite conclusions."

Bennett, seeing the vessel amidst a quantity of granules, believed that he had to deal, not with a true fatty degeneration

of the coats of the vessel, but with an envelopment of the arterioles, by granules and exudation-corpuscles. Whereas Paget, Leubuscher, and Hasse, Dr. Bouchard tells us, thought that they saw the adventitious tissue enveloping the matter, or the fat-granules in the wall of the vessel, and they, not knowing of the existence of a lymphatic sheath around the vessels, erroneously concluded that the fatty degeneration took place at the expense of the middle tunic.

The fat-granules occupy the lymphatic sheath, and Dr. Bouchard thinks that the fatty change, which the authors above referred to observed in the small vessels of the brain, was developed subsequently to the cerebral lesion, and was not the cause of the hæmorrhage. And he adds, "I believe, then, that I am right in formally denying the part which many authors, notably Paget, Hasse, Todd and Eulenberg, have ascribed to it in the production of hæmorrhage." He nevertheless acknowledges that primary fatty changes do occur in the capillaries of the brain; but, at the same time, holds that these have been confounded with the above-mentioned secondary metamorphoses.

It might here be contended that Dr. Bouchard merely surmises that the fat-granules observed by Paget and others occupied the lymphatic sheath; it might equally be said that these observers also refer to the other fatty change which Dr. Bouchard himself looks upon as a primary condition, but of which he fails to give us in detail the distinguishing features.

It seems to us that intra-cranial vessels, capillaries more especially, have their walls, not simply their peri-vascular covering, studded with so-called "fat-granules," and that in cases where there is seemingly no softening whatever. Sometimes these granules have apparently, by juxtaposition, amalgamated and formed larger "fat" collections. We have thought that in some vessels we could make out these collections extending through their wall, but more often we could not satisfy ourselves whether they were mainly or even entirely in the outer part only. As the matter stands, not a few of us, perhaps, would endorse Dr. Bouchard's conclusion, that this primary fatty degeneration or metamophosis needs still further observation.

Virchow says that he has seen fatty vascular degeneration in cases of chlorosis, and we also have observed extensive fatty degeneration of the muscular fibres of arterioles taken from a body in which there was a large pale, soft, fatty liver. Moreover, like Dr. Bouchard, we have noticed the vessels of young children studded with fat-granules, and in fact have observed the

like in the vessels of so many bodies, after death from various diseases, that we are in doubt as to their origin, or significance. In the walls of many of these capillaries there were "fat-granules" scattered here and there in a manner which led us to suspect that they were constituents of the blood which had transuded during dying or after death.

Dr. Bouchard, in respect to fatty degeneration of the vessels, concludes that it is not sufficiently marked to permit of its being regarded as a potent cause of hæmorrhage; and he considers that cerebral hæmorrhage frequently occurs in individuals who present no trace of this change. He might have added further, that hæmorrhage also occurs when, altogether apart from softening or encephalitis, this fatty change is well marked and largely diffused in the capillaries and smaller arteries. Nevertheless, with so much apparent "fatty" alteration in the wall of the vessel, it is difficult to conclude that it has no part, and even more difficult to define what part it plays in the causation.

It seems to us only reasonable to conclude that, if the fatty condition be really part of the degenerative process, it may be one of the disorganizing changes which lessen the resistance of the vessel; but granting this, we must again repeat, further observation is needed to show what action this fatty change plays in the rupture, if it be of itself sufficient so to disorganize the vessel that it can no longer resist the pressure of the blood.

The vessels at the base, in the fissure of Sylvius, and in other parts of the brain, have so often been found atheromatous in cases of cerebral hæmorrhage that a conviction has grown in the pathologist's mind that the two conditions, atheroma and hæmorrhage, are intimately connected as cause and effect. And this conclusion has, doubtless, been strengthened by observing the consequences attendant on atheroma of the larger vessels. Noticing that the inner coat of the artery was ruptured in some cases by the atheromatous change, the middle coat destroyed, and the blood retained by the adventitious coat only; whilst, in other cases, a portion of the wall of the vessel thus altered bulged aneurismally, or was even ruptured, it seemed probable that similar changes in the atheromatous arteries of the brain should produce sanguineous apoplexy.

This conclusion was generally, we might say universally, adopted by pathologists; and Niemeyer probably expresses current opinion when he, speaking of sanguineous apoplexy, dogmatically states, that the structural changes in the walls of the vessels, to which their abnormal fragility is due, in most cases, to endoarteritis deformans. Nevertheless, it has never

been shown that the ruptured vessel in cases of cerebral hæmorrhage is generally atheromatous; and we are, therefore, not surprised to find that M. Durand-Fardel considers that the influence of atheroma has been theoretically admitted without a proper examination of the facts; and that other observers have not only called in question the fact, but actually refused credence to it. Dr. Bouchard tells us that the authors of the 'Compendium de Médecine Pratique' deny altogether the influence of atheroma.

We are further informed by Dr. Bouchard that MM. Charcot and Vulpian have given him particulars of thirty-nine cases of cerebral hæmorrhage in which the condition of the arteries of the brain was observed. In seven cases, between the ages of sixty-two and eighty-four, they were not at all atheromatous; they were slightly or scarcely at all so in eleven, aged from fifty-three to eighty-one years; in thirteen cases, from the age of fifty-three to eighty-four, they were atheromatous in some degree; whilst in eight cases, from the age of sixty-six to eighty-eight years, they were very atheromatous. Leaving out he eleven cases in which the arteries were slightly atheromatous, there were seven out of thirty-nine in which the cerebral arteries were free from atheroma, that is, 18 per cent.

Dr. Durand-Fardel's statistics make the proportion of cases free from atheroma to be about 12 per cent. Whilst admitting that atheroma occurs more frequently in individuals who die of cerebral hæmorrhage than in those who present no disease of the brain, although the difference is not very great, Dr. Bouchard concludes that, the absence of atheromatous change in 18 per cent. of the cases of cerebral hæmorrhage occurring in old persons shows, that it is not the essential cause of sanguineous apoplexy, and that it acts only as an auxiliary or predisposing cause.

We now come to the leading subject of Dr. Bouchard's treatise, a "hitherto undescribed change of the small arteries of the brain," which M. Charcot and he consider the most frequent cause of cerebral hæmorrhage.

This morbid change in the arteries tends to the production of aneurisms, but is not limited to the vessels which are the seat of aneurismal dilatation. On the contrary, it is in some cases diffused through the entire system of small intra-cerebral vessels.

The principal morbid changes take place in the most external parts of the arterioles. The authors cited have found that the lymphatic sheath involving the vessels may present a wavy appearance, like a bundle of subcutaneous cellular tissue; or that there is in the sheath a large excess of spherical, or ovoid,

or slightly irregular nuclei. Sometimes these nuclei, which are seemingly connective-tissue growths, are present in such prodigious numbers, that the subjacent coats of the vessel cannot be properly seen. In the cavity of the perivascular sheath a few cells, fat-granules, and hæmatoidin-grains, only may be seen. The adventitious coat of the arterioles may be simply thickened, sometimes very greatly so, and its substance streaked like a bundle of fibrous tissue, whilst fusiform cells are seen lying in the direction of the long axis of the vessel. More frequently there is no such thickening, simply a large increase of connective-tissue nuclei in the tunica adventitia.

The muscular coat also is characteristically altered. The muscular fibres generally, or only here and there, are separated, thinner than normal, and in some parts of the arterioles they have disappeared entirely. This atrophy of the muscular fibres, the authors remark, is not primary; it seemingly follows the more external changes. They are led to this conclusion by observing that the "periarteritis," by which term they designate this disease, is often seen limited to the sheath and adventitious coat, the muscular then showing no appreciable change; and, moreover, the "arteritis" is most marked in those parts of the vessel where the muscular elements are most defective or altogether wanting. They find also little or no morbid alteration in the tunica intima of the diseased vessels; the only change there observed consisted in a multiplication of the large ovoid longitudinal nuclei of that membrane.

This "sclerous peri-arteritis," which the authors also call "arterial sclerosis," occurs in different degrees of intensity. It was always found in brains containing small so-called "miliary aneurisms," even where there was no cerebral hæmorrhage. But Charcot and Bouchard have in some cases seen this "sclerous" vascular change in cases in which there were neither hæmorrhagic cavities nor aneurisms. These authors then go on to inform us that, if the adventitious coat is not thickened and fibrous, the vessel, in the parts where the muscular coat is atrophied, is dilated in the form of ampullæ, which, say they, is the origin of the minute so-called "miliary aneurisms."

MM. Charcot and Bouchard, in alluding to "miliary aneurisms," expressly state that they exclude larger aneurisms, such as were seen by Vieussens, referred to by Majendie, and have been well described by Serres, by Gull, Gougenheim, Lebert, and others. These latter occur in the larger arteries of the base and of the meninges, and produce mainly meningeal hæmorrhage. The aneurisms the authors allude to are very much smaller, and although visible to the naked eye require a lens to be seen

distinctly. These miliary aneurisms are generally of a purplish, or reddish-brown, or ochrey colour; if their walls are thickened by connective-tissue growth, or white blood-corpuscles aggregated in them, the aneurisms have a greyish appearance. They may be firm and hard like sand, or very soft and easily lacerated. They occur in almost all parts of the brain, but most frequently in the corpus striatum and optic thalamus, and, following the order of frequency, in the pons, in the convolutions, and in the cerebellum. The authors sometimes found only two or three, but at other times more than a hundred in one brain. They have met with these aneurisms at all periods of adult life from the age of twenty upwards. Heschl found that after forty the rate of development of these miliary aneurisms is almost invariably a gradually increasing one, whilst before forty these lesions were rare. Cases are referred to, however, in which they occurred between twenty and thirty—one at the age of twenty-four, another at twenty.

In respect to these aneurisms, we may here remark it is now fully recognised that, in cases of cerebral hæmorrhage, the intra-cranial arterioles are commonly aneurismal; and it is presumable that the dilatation and thinning of their walls in very many cases determines the hæmorrhagic extravasation. In our experience the ampullary and fusiform dilatations are especially common; and, as MM. Charcot and Bouchard say, the muscular fibres of the dilated portions are much wasted or even lost, and the other layers may be also much thinner than normal. Nor are these aneurisms limited to the intra-cranial vessels. M. Lionville and others have found them in the vessels of the œsophagus, heart, retina, and spleen. Aneurismal dilatation of the arterioles of the spleen we believe to be not uncommon, whilst the like state of the retinal vessels is frequently demonstrated by the ophthalmoscope.

It is, perhaps, not difficult to understand why these intra-cerebral miliary aneurisms more especially give rise to large blood extravasations. For not only are the intra-cerebral arterioles thinner, but they have apparently not so thick a perivascular connective layer of tissue outside their adventitia as arterioles elsewhere; consequently, they have not so much fibroid thickening when diseased as is usually seen around the sclerosed vessels of the kidneys and other organs.

In describing the relation that these aneurisms bear to "sanguineous apoplexy" the authors mention that they collected 100 cases of cerebral hæmorrhage, and miliary aneurisms were found in every one of them, that is, in 100 per cent.; and they succeeded in one or two cases in finding an aneurism which had ruptured on the side opposite to that by which the

artery opens into its interior; it was, moreover, seen that the extravasated blood had escaped from the lymphatic sheath, which was also ruptured. Their researches likewise have led them to conclude that these aneurisms are always present in the brains of persons who die of cerebral hæmorrhage; and that, "of all the organic conditions capable of playing a part in the pathology of cerebral hæmorrhage, one only, by reason of its constant presence, appears worthy to be regarded as the direct cause of sanguineous extravasation, and that is the existence of miliary aneurisms."

Before we leave MM. Charcot and Bouchard's treatise we may, perhaps, usefully mention that Dr. Maclagan, in a footnote of his translation, remarks, "If the condition described by Sir William Gull and Dr. Sutton is found to exist, one cannot fail to see a possible connection between their hyalin fibroid changes in the adventitia and the chronic peri-arteritis of Charcot and Bouchard." And on turning to Sir William Gull's and Dr. Sutton's published papers it would seem that they observed in the arterioles of the pia mater and of other parts fibroid changes very similar to the arterial sclerosis of MM. Charcot and Bouchard; and although the term "hyalin fibroid" is used by the former, and sclerous arteritis by the latter observers, yet on putting aside these words, and being guided by their separate descriptions of the appearances, it would seem that they have, excluding the aneurisms, observed the same vascular disease. Both record that they found the tunica adventitia thickened, the muscular layer wasted in varying degree, and the tunica intima little affected. The differences in statement observed would seem to be dependent on the situation of the arterioles examined, the one studying cerebral hæmorrhage and the other chronic Bright's disease; yet knowing that cerebral hæmorrhage is most frequently associated with granular disease of the kidney, it would seem that the same class of cases was examined by both. Dr. George Johnson, however, as is well known, calls in question these hyalin fibroid changes; he thinks the muscular layer, where there is renal disease, is not atrophied, but, on the contrary, hypertrophied. But whilst recognising these seeming discrepancies, we cannot fail to see that there is evidence clearly showing that the arterioles of the body, after the age of fifty especially, commonly undergo degeneration, and the changes noted in the outer part of the arterioles supervene. But there are other morbid changes which begin in the inner part of the vessels and extend outwards, and also morbid changes in the capillaries which probably are likewise in some degree direct or indirect causes of cerebral hæmorrhage.

In some cases, especially in aged wasted bodies, we have observed the arterioles very thin, seemingly atrophied generally, but we are unable to say if this change had any causative relation to cerebral hæmorrhage.

With regard to the foregoing vascular morbid conditions which may coexist in the brain, it is, in our present state of knowledge, impossible to recognise rightly their individual influence in determining cerebral hæmorrhage. But opinion seems to point to atheromatous change as a common cause of meningeal hæmorrhage, to peri-arteritis as the most frequent producer of intracerebral hæmorrhage, and to fatty degeneration of the capillaries as the most common cause of capillary hæmorrhagic exudations.

Having thus far considered the alterations in the arterioles and capillaries which lead to cerebral hæmorrhage, we may now pass on to notice the other causative internal conditions which determine it.

Past observers, impressed with the fact that cerebral hæmorrhage and cardiac hypertrophy frequently coexist, and being unacquainted with the minute arterial changes considered above, attributed the vascular rupture mainly to the undue action of the hypertrophied heart. As knowledge of the vascular lesions increased, however, that belief diminished, and now we find the authors before us state that the hypertrophy is only accessory, and plays only a secondary part in the causative phenomena. But it would seem that we are still very far from being able to define the precise effect of the hypertrophy.

We are led to make this remark by observing that it is yet undecided what gives rise to the cardiac thickening, and it is further obvious that the effect on the arterioles will vary according to the position of the obstruction in the vascular system. If, as some believe, the blood, owing to its morbid condition, is greatly impeded in its passage through the capillaries, then the action of the hypertrophied left ventricle will obviously be felt throughout the entire arterial system, unless we accept Dr. Johnson's theory, and conclude, on the contrary, that the muscular hypertrophy in the arterioles counteracts, as it were, the increased power of the heart, and thus saves the very minute capillaries from undue pressure. If, however, the impediment be in the larger arterioles, then the increased impulsion would extend to the seat of the obstruction only.

It is now also thought by pathologists that the diminished elasticity of the larger vessels must be followed by a pulsatory jerking action in the arterioles and capillaries; if this be so, their walls must be exposed to injurious strains and their rupture facilitated.

Anything obstructing the return of venous blood from the brain is also considered by some observers to be an agent in the causation. Dr. Jackson, however, points out, and he quotes Niemeyer in support of his statement, that in the last stages of emphysema there is venous hyperæmia of brain, and yet in such cases cerebral hæmorrhage is rarely seen; and this he attributes to the fact that, as the blood collects in the venous system, the arterial is "under-blooded" and not strained. But whilst recognising such to be the case, a further consideration of the facts may show that the conditions are more complex. It is common enough in these and other extreme cases with great venous engorgement to find evidence of capillary hæmorrhagic extravasation, but not large hæmorrhages. If, however, instead of selecting cases in which the arterial system is greatly depleted by this accumulation of blood on the venous side, we, on the contrary, take cases in which there is only moderate, or but little venous congestion of brain, in such the backward accumulation in the venules and capillaries may obstruct the flow of the blood from the arterioles; the heart at the same time acting very irritably and unduly, —which it probably would do under such conditions,—the arteriole tension would be raised; the consequence would be that the walls of the arterioles would be irregularly and unwontedly strained and their rupture favoured.

All observers are agreed that the kidneys are diseased in by far the majority of the cases of cerebral hæmorrhage; and it is thought that the failure of renal function renders the blood impure, consequently there is increased resistance to its passage through the capillaries, and the arterial tension being in that way increased, the renal disease tends to bring about rupture and hæmorrhage. But to what extent the disease in the vessels themselves causes the augmented action of the heart, and increased strain on the vessels; and what is the precise action of this impure blood, are questions yet to be determined.

To recapitulate: it would seem probable that several conditions concur to bring about eventually the rupture. The diseased portion of the arterioles becomes much thinned, aneurismal, and less resistive, and there is some augmented contraction of the left ventricle, additionally and abruptly increased, perhaps, by exertion, by straining, &c., and at the same time, owing to the loss of elasticity, the heart's impulsion is unduly transmitted to the arterioles, perhaps even to the capillaries. Moreover, there may be increased tension of the arterioles from venous obstruction, and possibly from obstruction to the flow of serum from the capillaries, whereby the tension will be still further increased. Cerebral hæmor-

rhage, however, does occur in some cases in which there is little or no cardiac hypertrophy. It is, therefore, presumable that a diseased vessel may be ruptured by the blood pressure from a normal left ventricle.

We now proceed to inquire what are the clinical features of cases of cerebral hæmorrhage the authors describe.

It is now recognised that a person at the time of the blood effusion may have an apoplectiform or epileptiform seizure, or he may become paralysed without loss of consciousness.

Jaccoud and Hallopeau say, "dans la grande majorité des cas le debut est apoplectique." They tell us that the patient is seized abruptly, loses consciousness, and falls; he is insensible to all irritation, his limbs lie helpless, and when raised they fall inert. Dr. Lidell remarks that, in occasional instances, cerebral hæmorrhage produces the phenomena of apoplexy. Trousseau states that for more than fifteen years his attention had been directed to this point in the history of cerebral hæmorrhage, and he never had the chance, never once, of seeing a patient struck down suddenly by apoplexy, in the classical and etymological sense of the word; and he goes on to say that he had seen a great number of individuals suffering from cerebral hæmorrhage in the most profound apoplectic stupor; but in every case without exception, when the attack had occurred in the presence of witnesses it had been noticed to come on gradually, and had in general been slight at the outset, coma supervening ten minutes, half an hour, an hour, or several hours afterwards. Dr. Jackson, after saying that a person may become apoplectic almost without previous symptoms, remarks, "I say almost because, however quickly apoplexy from cerebral hæmorrhage comes on, there are nearly always some previous symptoms"—something wrong before the patient becomes unconscious, and often the interval is considerable—minutes or even hours. Such is our experience also.

The incidents at the outset are not unfrequently as follows. A patient is seized suddenly with severe pain in the head, or he may even feel as if somebody had knocked him on the head. We remember the case of a man who, whilst working, suddenly fell; he almost directly got up again, and accused a fellow-workman of having knocked him down by a blow on the head. After this he became confused, subsequently comatose. Post-mortem examination showed no signs of injury but a large hæmorrhage into one hemisphere and into the ventricles. Or the patient may first notice that his arm is numb, or that he has a sense of great heat in his arm, or in his leg; or he feels that he is losing the use of "his side;" a few seconds, minutes, or may be some hours afterwards he may become un-

conscious. But in some cases, undoubtedly, no history of such preceding symptoms can be obtained, but even with such cases we might with Trousseau ask, "Who knows how the attack sets in?"

Trousseau admits that a person may suddenly become unconscious and remain so until death; and he shows, what is fully recognised, that some patients attacked with hæmorrhage into the pons Varolii sometimes suddenly become apoplectic. But here also it is often impossible to obtain any information to show whether there were or were not any antecedent symptoms. The only particulars obtained, perhaps, are somewhat as follows:

"The patient was riding in an omnibus, supposed to be well, when she suddenly fell off the seat and became insensible. She was immediately taken to an hospital close by, and found on admission to be completely unconscious, her pupils much contracted, her breathing stertorous and irregular. The next morning she dies comatose, and the post-mortem examination reveals hæmorrhage into pons."

But it is equally beyond doubt that some persons attacked with hæmorrhage into the pons do not at once become apoplectic. Dr. Jackson gives particulars clearly establishing this fact. He mentions the case of a man who came off a scaffold because he was giddy, and then he lost his consciousness. Also just before the seizure another patient, whilst taking a drink, cried out she was poisoned; and he tells us Dr. Hare relates that a patient was able to knock at a door and say she was going to die before she became insensible. Yet in the pons of each of these patients there was hæmorrhagic extravasation.

It is now known that it is still more rare for cerebral hæmorrhage to cause apoplexy and immediate death. Sudden and immediate death by apoplexy does not indicate cerebral hæmorrhage. Rapid—in a few minutes—death occurs in some cases of meningeal hæmorrhage, or, as just said, with bleeding into pons, but we have on different occasions carefully examined the brains of persons who had seemingly suddenly and immediately died of "apoplexy," but in not a single instance did we find cerebral hæmorrhage. Their brains were apparently atrophied only, or, to the naked eye, not appreciably diseased, and death in some cases was seemingly due to renal or cardiac disease.

In other cases the hæmorrhagic effusion is ushered in by epileptiform symptoms. The patient falls down, or otherwise passes into "a fit;" and Trousseau says that, whenever cerebral hæmorrhage begins with an epileptiform attack, apoplectic stupor will set in suddenly, as it does after epilepsy. Sometimes the convulsive seizures are repeated several times, or even recur

until the patient dies. They may occur on one or on both sides of the body, or there may be hemiplegia of one side and convulsive movements of varying degree of intensity on the opposite side, accompanied, perhaps, by spasmodic movements on the paralysed side.

It is usually supposed that the convulsive seizure is excited by the extravasated blood making its way towards or into the ventricles, or towards the surface of the hemisphere. Dr. Jackson believes that convulsions occur, not because blood is effused into the ventricle, but because the clot is a large one and has suddenly torn much of the brain.

Further, the hemiplegia may set in without loss of consciousness, but in an hour or two afterwards coma supervene. We have particularly in mind a case of this kind. A man, whilst walking along the street, suddenly felt something wrong in his head, and fearing that he was going to be ill, walked home. When seen about an hour afterwards by a medical man, he was able to give a clear statement of what he felt and when the attack began. After making this statement he become comatose and died. The autopsy revealed an enormous blood extravasation which had burst into the ventricles, almost entirely destroying one hemisphere. Such a case shows that a very large effusion does not necessarily give rise to apoplectic phenomena.

We have already mentioned that the remains of hæmorrhages are not unfrequently found during post-mortem examinations, and from their appearance it would seem that the quantity of blood extravasated is sometimes very small indeed, not larger than a small pea, or even smaller, and it occasionally happens that the history of the case may enable us to connect the brain change with some sort of seizure. We remember a woman about the age of fifty telling us, whilst giving an account of her sufferings, that one day she was standing by the side of a tub, washing clothes, when all of a sudden, without any cause that she knew of, she suddenly fell on the floor; she got up by herself, felt confused, but was soon able to resume her work. A year or so after this she died, and we carefully examined her brain and found in one part a small cyst, such as is seen after hæmorrhage effusion. Dr. Jackson says, "We may have no symptoms from small clots in the hemispheres." If we understand him rightly, he means we may fail to find any symptoms, and the patient perhaps may not have noticed anything he thinks worthy of recollection or of speaking about.

Having now considered the manner in which the hæmorrhagic seizures are ushered in, we may briefly notice the symptoms which directly follow and result from the injury to the brain.

Although the appearance of a patient suffering from apoplectic stupor is very familiar to all of us, we are tempted to give MM. Jaccoud and Hallopeau's description because it affords a faithful portrait.

The patient's face is often congested, at other times very pale, without expression; the cheeks flap regularly with each expiration; swallowing is often difficult; and the reflex movements of the soft palate and pharynx are much weakened or abolished, and some of the liquid given falls into the air passages and produces cough. Respiration is at times accelerated, and at other times slower than usual. The face and extremities have a bluish appearance. The patient makes no effort to get rid of the mucus accumulating in his trachea and bronchi, and the air respired, traversing the air passages, produces stertor. The pulse is often slower than normal in the early period of the attack, later on it is accelerated, and becomes small and irregular. Sometimes vomiting occurs, and often the urine and fæces are passed involuntarily. The temperature falls considerably; it may even in the rectum be only 97·1; but usually some hours after the commencement of the seizure it again rises, and after a few, that is, two, three, or four days, it rises sometimes several degrees above the normal standard.

Dr. Jackson truly points out that the voluntary movements fail first, suffer most, and recover latest. The automatic movements of belly and chest may be weakened, but they are frequently regained in a few hours or days. There is a difference of opinion as to the degree in which the abdominal and thoracic muscles suffer; but all would agree that in many cases it is difficult to recognise any failure of power in them.

We need hardly say that the several authors agree that in ordinary hemiplegia the lower part of the face only, the tongue to some extent, and the arm and leg, are paralysed on the same side. And in less severe seizures the facial paralysis may be hardly appreciable, the leg also may be very little affected, and the patient may only allude to the loss of power in his arm.

It was years ago pointed out that the eyes are not unfrequently turned towards the non-paralysed side, and M. Vulpian says the head also is similarly turned. Dr. Jackson has, moreover, observed that frequently both upper eyelids fall for a short time. The eyes with this lateral deviation are parallel, and Dr. Jackson has found that some patients are able to bring them to the middle line or even further, but they soon fall again into the deviating condition. Generally this deviation continues only a few days. Ordinarily, as Jaccoud and Hallopeau say, this symptom is transitory, but they have known it to remain persistently for several months, and they think that it is

especially present when the corpus striatum is the seat of lesion, although it has been seen with meningeal hæmorrhage. Observers find it difficult to account for this deviation, but it is probably due to some of the effused blood, or its serum, making its way down from the corpus striatum or the thalamus into the crus and even further. That the blood does so sometimes may be demonstrated; and in so doing it might press on the third nerve of the eyeball opposite to the hemiplegia and the external rectus, consequently turn the eye outwards away from the paralysed side; whilst the sixth nerve at its origin on the same side as the brain lesion is injured before it decussates, therefore the external rectus on the hemiplegic side being paralysed, the internal rectus turns the eye inwards and away from the paralysed limbs.

The lesions in the brain resulting from hæmorrhage are generally diffused, and when seen are mostly widespread. There is not usually one small part affected, as is the case more frequently with growths in the brain. Therefore hæmorrhagic cases afford fewer opportunities of studying the precise effect of local injury. In saying this we have especially in view the cerebral ganglia. For instance, if the corpus striatum be injured the thalamus is often invaded by the blood or by serous infiltration; how far, therefore, the paralytic or other symptomatic disturbance be dependent solely on changes in the corpus striatum or thalamus is still a matter of doubt.

Dr. Jackson thinks that the arm suffers less when the back parts of these ganglia are the seat of hæmorrhage, and he agrees with MM. Jaccoud and Hallopeau, that with lesion of the thalamus sensation is lost in the hemiplegic side, although, perhaps, only for a few days. Other observers, however, question the fact.

Not unfrequently physicians have an opportunity of studying the symptomatic disturbance resulting from copious hæmorrhage into the pons Varolii. Usually when seen the patient is lying on his back, breathing laboured, the epigastrium sinking in during each respiration. The colour of face may be normal and the eyelids dropped, pupils very small, and on raising the arms and legs they fall like "dead weights;" and when the conjunctiva is touched there is not the least sign of sensibility; the patient is completely comatose, and before many hours are passed the autopsy usually reveals a clot in the middle which almost occupies the entire pons. In other cases the hæmorrhage is on one side of, and in the lower part of, the pons, and then there is hemiplegia on the opposite side, and very marked facial paralysis on the same side as the lesion. Dr. Jackson, referring to Dr. Brown-Séquard's lectures for evidence, argues

that a lesion in the upper part of one half of the pons, let us say the right, causes paralysis of the left side of face and of the left limbs, because the fibres of the left facial are injured before they decussate, and the peduncular fibres also; and a lesion in one half of the pons may injure the fibres of one facial before they cross, and the fibres of the other after they have decussated. Hence there is double facial paralysis and hemiplegia on the side opposite to the lesion. And these observations we are told apply to the fifth nerve also; when this last is involved the face is anæsthetic, the masseter and temporal muscles are paralysed; whereas in a more limited lesion of one half of the pons, the motor fibres of the fifth only may be affected. If there be extensive hæmorrhage into the pons both facials and the fifth nerve may be involved.

When the portio dura is paralysed by lesion of the pons there is, Dr. Jackson asserts, often paralysis of the sixth nerve also on the same side; the opposite sixth, moreover, according to Brown-Séquard, may also be affected in a greater or less degree. Dr. Jackson explains this coincident involvement of the sixth and seventh by referring to Stilling and Lockhart Clarke's researches, from which he shows that, although the facial and sixth nerves emerge at different places, yet they arise from a common nucleus.

Hæmorrhage limited to the crus cerebri is much more rarely seen. It produces the symptoms now recognised to occur in lesions of this part; namely, hemiplegia on the side opposite to the lesion and paralysis of the third, with ptosis on the same side as the hæmorrhage. There is loss of motion only if the under part of the crus be injured, but if the upper part be the seat of hæmorrhage, there is likewise loss of sensation.

Dr. Jackson usefully points out that we can only make the diagnosis of injury to the crus when the palsy of the third nerve and hemiplegia come on simultaneously. If they come on at different times it is quite as likely that there are two lesions, one of the optic thalamus or corpus striatum, the other involving the third.

There is little indeed to be said of hæmorrhage into the cerebellum,—there may be loss of consciousness at the time of seizure or not. In some cases there is hemiplegia on the same side as the hæmorrhage; in other cases, on the opposite side, or even no paralysis at all. Sometimes we are told that there is conjugate deviation of the eyes,—one eye turned upwards and outwards, and the other downwards and inwards. With this symptom we may diagnose, Dr. Jackson thinks, sudden lesion of the crus cerebelli. We recollect a case of hæmorrhage into the cerebellum, in which the only prominent

symptom was severe pain in the back of the head. The patient died of lung disease and the cerebellar blood extravasation was discovered during the autopsy.

Still less can be said of hæmorrhage into the medulla oblongata. It is admittedly exceedingly rare. Dr. Jackson refers to an interesting case. A patient having had an attack of hemiplegia, afterwards discovered to be due to clot in his right optic thalamus, all at once lost power to articulate, from paralysis of the tongue, but he was able to write. A few years afterwards Dr. Lockhart Clarke found the remains of a small hæmorrhage in this patient's medulla oblongata.

If there were a lesion of the medulla oblongata, paralysis of lips, tongue, palate, and vocal cords would probably occur. We once had an opportunity of examining the body of a patient in whom these symptoms came on slowly, and we found a tumour in the medulla.

Having thus far noticed the leading clinical phenomena observed during and after sanguineous apoplexy, we may now usefully consider the symptoms premonitory of cerebral hæmorrhage.

Dr. Lidell's remarks on these symptoms fill eight pages, but we confess that we have learnt very little from them. He does not sufficiently discriminate between the symptoms which often precede apoplexy and those that are premonitory of hæmorrhage into brain. The older physicians, as he shows, fully recognised that persons subject to apoplexy were commonly troubled with vertigo, headache, fulness of the head, confusion of thought, failure of memory, noises in the ears, disturbed sight, restless nights, irritability, paræsthesia, anæsthesia, &c., but they did not know that many different lesions of the brain, also disease in other organs, notably renal, caused apoplectic phenomena.

The above-named symptoms are more or less common to all forms of brain disorder, and after the age of forty or fifty they are frequently met with in connection with various morbid conditions. It is often very difficult—in fact in the present state of our knowledge perhaps impossible—to appreciate them, so to speak, at their proper value, or rightly know their origin. The symptoms themselves denote disturbance in the cerebro-spinal centres; and it is common to find, especially after the fiftieth year, indications of local disease in the eyes or ears, in heart or liver, kidneys or lungs, stomach or uterus, singly or conjointly in several of these organs, or even in all more or less, and signs also of tissue degeneration. And the question arises how far is the disturbance in brain function consequent on the local change, and how far is there coexistent, allied, or

independent disease in the brain itself? Sometimes also we have to consider if the disturbance in the stomach, heart, or other organs be the result of the brain disease.

Until it be shown that these symptoms are dependent on brain change, which especially leads to hæmorrhage, and that they occur in a very large proportion of cases a little before hæmorrhagic seizures, it seems to us that it would be misleading to regard them as premonitory.

Some observers after special inquiry have come to the conclusion that there are no reliable symptoms premonitory of cerebral hæmorrhage. Of course they simply mean that they have failed to find them in many cases.

Jaccoud and Hallopeau say, "rarement des prodromes précédent le debut de l' hæmorrhagie; ils n'ont généralement rien de caracteristique." Likewise Dr. Jackson, speaking of the premonitory symptoms, says, "We must remark that some patients who die of cerebral hæmorrhage have had none, at least we hear of none: and this is sometimes the case when examination both during life and after death reveals signs of most extreme degeneration."

We also have looked for warning indications and failed to find any in some cases, but knowing that it is very common to observe the remains of small extravasations which have evidently occurred in the brain some time before the larger effusion, it would seem probable that there are commonly slight seizures of some kind or other before the more severe ones; and if the blood escapes from an aneurism it would seem probable that cerebral, like many other aneurisms, would rupture first, slightly, and one or more small extravasations precede the large fatal effusion.

If a person with well-marked signs of arterial degeneration be seized with symptoms, no matter how slight, indicating sudden lesion in the conducting fibres of the brain, probably most physicians would regard the symptoms as premonitory, and conclude that the patient is very liable to have a more severe attack. But they might differ in opinion as to whether the disruption was due to softening or to hæmorrhage; for it is now generally recognised that even with softening the symptoms may seemingly set in very suddenly, as in hæmorrhage.

The premonitory symptoms which occur and indicate such disruption are, sudden weakness in one arm or leg, maybe an arm only, or probably the sudden onset of spasmodic movements in the arm and leg. Or, again, there may be no appreciable loss of motive power, but disturbance in sensation only. A patient may tell us that days, or even weeks before the severe hemiplegia, a sensation of great heat extended suddenly

through his arm and leg, or one hand and foot felt "so cold." The sudden occurrence of facial paralysis affecting the lower part of face, and passing off in a few days, is another unfavorable sign, for, as Dr. Jackson says, "it shows cerebral lesion. And he further tells us there may be loss of speech with the above-mentioned kind of palsy, more often defect of speech only,—a difficulty of articulation which the degree of palsy of face and tongue does not account for, and the patient expresses himself as badly in writing as he talks." And here again there is evidence of cerebral disease, probably a small lesion near the floor of the fissure of Sylvius.

There may be in the place of these symptoms sudden loss of consciousness, or even an epileptiform seizure, or there may be signs of vascular rupture, namely, a profuse attack of epistaxis or retinal hæmorrhage, with sudden defect of sight.

Of course it is the sudden occurrence of one or more of these symptoms in a person with vascular disease and with no signs of other morbid condition known to give rise to these local signs, which makes the symptoms strongly presumptive of cerebral hæmorrhage. It is by a process of exclusion that these signs acquire their value.

It remains for us in bringing this notice to a close to briefly recall the morbid actions and effects which occur in these cases of cerebral hæmorrhage, and endeavour to realise the consecutive changes. Although experience has shown that a person seemingly in ordinary health may die "suddenly" or rapidly of cerebral hæmorrhage, meningeal especially, and that the examination after death may reveal merely extravasated blood and a ruptured aneurism, there may be apparently very little vascular degeneration, and what there is, is very localised. Such cases, however, are comparatively very few indeed. There are nearly always slow, appreciable degenerative changes long antecedent to the hæmorrhage. It would seem that these may begin in any part, or, about the same time, in several parts of the body. Not unfrequently a patient may tell us that he was well before the attack, but we have only to examine him to recognise that his statement means he did not know he was not well.

We may first notice that he loses his healthy look, his skin loses its elasticity and smooth appearance; it often becomes greasy looking, its colour also alters. The patient may be conscious of change, but complain only of dyspeptic disturbance, maybe of constipation, or that he cannot work as he once did, and he is sooner fatigued. Subsequently we may find that he has a winter cough, even bronchitis, and he becomes conscious that his breath is "not so good as it once was." He may one day have a severe attack of epistaxis. Albuminuria may

then or at a much later period be discovered, and his heart then or before may be found dilated, on the left side especially. The pulse is harder, it strikes the finger more abruptly, and its beat often feels unusually long. His head troubles him, things disturb and harass him which at one time would have had no such effect, and at times he feels to himself almost unbearable. Not unfrequently there is more or less giddiness and an uncomfortable feeling in the head, not severe pain; his nights are disturbed. His sight may also get dim, his eyes are "more easily fatigued." Rheumatoid or gouty changes are common in such cases.

These signs of failing health often precede cerebral hæmorrhage as well as other morbid conditions which occur commonly in middle or advanced life.

In this way the decline occurring gradually, may extend over months or years. One day the patient may come and tell us that whilst he was sitting writing a letter the day before he "all of a moment" found that he could not find the words. He turned to speak to some one, and he could with difficulty make himself understood. He goes on to tell us that he was alarmed and went home, and on his arrival there he could speak "all right:" and there is no noticeable change next day, and he may smile whilst telling us of the oddity of the disturbance; or he may have sudden failure of sight due to retinal hæmorrhage, or, not feeling as well as usual, he thinks his stomach is out of order, and it is then noticed that his mouth is drawn to one side; there is temporary and incomplete facial paralysis; or he complains that he felt a sudden weakness or numb sensation in his arm or in arm and leg. In a week or two he may seemingly cease to notice the event, and goes on working as usual, and months after this he may suddenly become "faint," lose his senses, and be found hemiplegic.

And we may have an opportunity of seeing a large recent extravasation, and a small ochrey yellow change or cyst the remains of a smaller antecedent hæmorrhage. In these cases it is generally thought that there is a gradual degenerative change in the vascular system and elsewhere, that abruptly a capillary arteriole is ruptured, and the small blood extravasation produces the sudden slighter paralytic, sensorial, or motorial disturbance. The quantity of blood effused being very small, rupture of many nerve-fibres does not follow, and after a few hours or so the blood is diffused between the fibres and in the peri-vascular canals, whence it happens that the slight paralytic symptoms produced quickly subside; but as the arterial degeneration continues dilatation and rupture of the vessel are favoured; by the localised atrophy, as already said, in the wall

of the arteriole; by its lateral support being diminished; by the brain wasting, some add also by softening of the immediately surrounding brain substance; and rupture is promoted by the increased impulsion of the hypertrophied heart; and maybe by the increased tension caused by capillary or venous obstruction. And although the ruptured vessel can very rarely be discovered, yet all writers concur in the conclusion that rupture does take place because there is no other known way in which blood in such very large quantity can get out of a vessel.

At the moment of rupture it would seem there is almost always, if not always, when the blood rushes through the rent, a "shock," a "surprise," an interruption to brain function more or less severe.

If the rupture occurs in the pons where the conductors of the projective system are collected together in a small area, then even a comparatively small extravasation, tearing asunder and breaking in two the conducting nerve-fibres, may very rapidly destroy the means by which the convolutions communicate with the external world; and if the blood be rapidly poured out it is not difficult to understand that the apoplectic phenomena may develope very quickly indeed, not instantaneously, but in a few seconds or minutes. And seeing that in pons cases the hæmorrhage is very near the nerve-origin nuclei in the floor of the fourth ventricle, it would be surprising if death did not occasionally occur rapidly in some cases where the hæmorrhage is so situated.

When the extravasation is higher up in the projective system, mainly in the central ganglia, the symptoms tend to show that the shock may in the beginning be marked, yet be not so great but that the patient can walk or even describe his symptoms to some extent; or on the other hand it may be soon followed by coma, with or without convulsions.

If we stop to consider what is meant by "shock" it is clear that we all understand a "knock," a sudden abnormal movement extending through the brain substance. And this movement being disorderly it seems only reasonable to think that it throws the molecules of the nerve cells and fibres somewhat out of position, and according to its intensity must interfere with sensori-motor actions travelling to and from the brain. Hence the world without seems confused and the sufferer cannot consciously, certainly not clearly, communicate with it. As soon as the pressure of the blood extravasated is equal to the pressure of the blood within the ruptured vessel, then the bleeding, as Niemeyer says, must cease; and if the extravasation be not very great it is not difficult to conceive that the

remaining sound nerve-fibres may be sufficient in varying degree to conduct the afferent and efferent motions through the projective system to the convolutions; especially if we keep in mind the presence of not only the large commissural communications which generally escape injury until the blood bursts into the ventricles, but also the existence of the minute, more intricate, and vast commissural fibres between cell and cell. And thus the actions of the external world can be transmitted by these afferent and commisural fibres which have escaped injury to the perceptive volitional and sensory parts, but the ruptured nerve-fibres of the damaged part of the brain cannot transmit volitional actions, or can transmit them very imperfectly, the muscles supplied by their broken fibres are therefore paralysed. We may thus conceive, if we do not rightly understand, how consciousness and sensation generally return, whilst loss of motion, partial or complete, remains.

Dr. Lidell, following Niemeyer, refers the apoplectic phenomena to sudden compression of the cerebral capillaries, that is, to suddenly produced anæmia of the brain substance. He remarks it is usually supposed that the apoplectic fit is a result of the pressure or bruising of the nerve-filaments and ganglion cells of the entire brain by the extravasation; it is, however, evident that this pressure can never exceed that of the blood in the cerebral arteries; for as soon as the pressure on the parts around the arteries is as great as that of the blood in the vessels no more blood can escape from the latter. Such a degree of pressure, he thinks, would not destroy the conductibility of the nerve-fibres; he says, "from experiments that we can make on peripheral nerves there is no doubt that such a pressure is entirely insufficient to annul the excitability of the nerve-filaments."

To this it might be replied that even if it be shown that a degree of pressure equal to what occurs in the arteries is insufficient to annul the excitability of the peripheral nerve-elements, yet it is to be kept in mind that the intra-cranial nerve conditions, so far as is yet known, are very different to the peripheral; that the cerebral, more especially the convolution nerve-elements with all their pyramidal, spherical, and other cells, and their wonderful intercommunicating processes,—the supposed seats of conscious and volitionary actions, are seemingly much more finely constituted, and are probably, moreover, much less able to resist pressure than the peripheral nerve structures. And whilst it is evident that the pressure can never exceed that of the blood in the arteries, it is to be remembered that normally the pressure on the nerve-elements is less than the pressure within the vessels, otherwise how does

the serum flow out off, and away from the capillaries; but when the blood extravasated raises the pressure of the nerve-cells and filaments until it becomes equal to that of the blood, not simply in the capillaries but in the arterioles, then the condition of the nerve-cells and fibres is quite altered, and their functional activity most likely proportionately diminished or lost.

And we are further reminded by Dr. Lidell that Niemeyer holds, that if the symptoms of paralysis depended on the pressure to which the filaments of the brain are subjected in apoplexy, bleeding should remove these symptoms, not only in some, but in all cases; provided enough blood be drawn to lessen the pressure in the whole vascular system, particularly in the arteries. To this objection also it might be replied that if sufficient blood were withdrawn the pressure on the nerve elements must of necessity be diminished; but in taking the blood out of the body, the tissues of the brain are deprived of the blood-material and of motion requisite for their functional activity. Therefore whilst the pressure is being reduced the anæmia is being proportionately increased, and most probably the operator would have to report an oft-repeated tale,—that the operation succeeded, but the patient died.

To resume, it would seem that there are three conditions which mainly cause the apoplectic and paralytic phenomena, —a sudden abnormal movement through the constituents ot the brain, communicated by the blood flowing from the vessel,—so-called shock, and it is well known that sudden loss of consciousness, coma, and death may be produced by a blow, with few signs of contusion, and no evidence of squeezing. And we remember a case in which a blow produced unconsciousness, and a loss of power, much more marked on one side seemingly than on the other, and on examining the brain, expecting to find pressure, there was merely evidence ot contusion and not extensive laceration. If, therefore, a shock thus communicated through the skull, through the cerebro-spinal fluid, in which the brain may be said to "float at anchor," it would seem equally certain that blood suddenly ejected from an arteriole into the very substance of the brain, must convey a powerful shock, sufficient, perhaps to cause apoplectic coma.

There follows next a sudden compression of the brain because the greater degree of pressure of the blood from the ruptured arteriole is communicated to the nerve-fibre, cells, and capillary wall; and the brain elements are therefore squeezed before the cerebro-spinal fluid can be diminished; whereas in more slowly accumulating pressure there may be time for this fluid to be pushed away.

Coincidently with this compression the blood current through the brain is greatly diminished, by the compression of the capillaries, as just said; and in some cases also by the failure of the cardio-respiratory actions. For as the lungs become engorged, and they nearly always do so, another condition depleting the capillaries is brought into action.

Experience teaches that in cases which recover, the respiratory function is little or, at all events, not much impaired, which seemingly indicates that, in such patients, the cerebro-spinal respiratory centres are much less involved in the apoplectic commotion. In fatal cases, however, there is evidence to show that the medullary region is either compressed like the remainder of the brain; or blood, as is common, flows down through the third and fourth ventricles to the medulla and compresses the arteries and capillaries, entering and supplying the medulla, thus arresting its activity; or the blood or its serum penetrates down the crus along the pons tracts, and injures the nuclei in the floor of the fourth ventricle. However, be that as it may, it seems clear that death is produced by failure of respiration, and apparently in some cases by coincident failure of heart's left ventricle also.

It is notorious that the lungs are very much congested; dark masses like pulmonary apoplexy may be seen scattered here and there; and when the respiratory disturbance has continued several days after the apoplectic seizure, there may be masses of consolidation, like lobular pneumonia, some of which may even have softened, forming ill-defined cavities. Without raising the question of how much the respiratory function is governed by the fifth and eighth, and how much by the spinal nerves, we should perhaps say, lower nerves of the projective system, we may here call to the mind of our reader that Reid many years ago showed that the lungs when the vagus was divided were sometimes very much congested only; in other animals they were of a dark colour and dense; other lungs in parts were consolidated so as to present the conditions known as red and grey hepatization.

These changes sufficiently resemble what is met with after death by cerebral hæmorrhage to suggest, if not to indicate, that the congestion and other morbid conditions are caused by the brain lesion; and on examining such lungs the alveoli may be seen filled with cloudy granular and corpuscular matter, seemingly serum and blood-corpuscles, which here and there completely fill them: and this is the case not simply in the posterior parts. This exudation, seemingly by rupture or transudation from the distended capillaries, excludes the air and the circulation fails coincidently.

Before closing our notice of Dr. Jackson's article we may observe that, in describing the differential diagnosis, he says, that sometimes it is impossible from the condition of the patient when seen, and the absence of a history of his previous condition, to say whether the coma is the result of cerebral hæmorrhage or of some other brain affection ; of renal disease, or of poisoning by alcohol ; besides, when there is hæmorrhage in the pons the symptoms present are very like those of poisoning by opium. We might here add if a person were required to make a diagnosis in such comatose cases, in the entire absence of any history of the preceding symptoms, it would be like asking a man to tell the events of the declining day by the appearance of the setting sun.

We do not see anything calling for remark in the treatment recommended; it is what is usually adopted. Dr. Jackson says it is a good practice to give croton oil unless the alteration of the pulse, respiration, and temperature be extreme. He doubtless has good reasons for this opinion, but they are not evident on the surface.

In closing this review it only remains for us to say that Dr. Bouchard's treatise is well worth reading, for it not only gives in detail the results of M. Charcot's and his own valuable and original observations, but presents a careful analysis of the several changes in the brain, to which hæmorrhage has been and is still attributed. Dr. Maclagan has done good service in translating it ; the author's meaning he brings out clearly and it is a pleasure to read his translation. We hope that Dr. Lidell will revise his work ; it contains a good deal of valuable material, but his comments on his cases we cannot commend nor accept. True to his purpose he has written a work on apoplexy, but has also included in it an account of almost every known brain disease. Jaccoud and Hallopeau's article is well worth reading, and affords a valuable summary of the present day knowledge of cerebral hæmorrhage. Dr. Jackson's article has been rewritten, and it is the outcome evidently of his own personal observations ; the record of one who has tracked brain disease seemingly with great ardour, and following it long, knows well its course and character, and is consequently enabled to record his experience forcibly and clearly.

IX.—Tait on Diseases of the Ovaries.[1]

THE subject of this essay is one of such magnitude that its full consideration is impossible in the limited space which seems to have been at the author's disposal. Mr. Tait has, however, managed in a short space to convey a good deal of information, some of which, resulting from his own observations, is not without interest.

After a brief review of the anatomy and physiology of the ovaries, Mr. Tait passes on to the functional and inflammatory diseases affecting those organs, and the dysmenorrhœa resulting from them. In the treatment of these diseases Mr. Tait seems to look upon marriage as an important remedial agent. He recommends it for ovarian dysmenorrhœa with threatened atrophy of the organ, and for ovarian hyperæmia. Speaking of the former condition, he says, "Marriage is perhaps the most efficient remedy, and one we ought seldom to hesitate to recommend; for even if the patients may not have children they will have better health, and they may even become pregnant if they marry early enough, and are not mismanaged." Again he tells us that "marriage, even without pregnancies, seems to modify the menorrhagia (of ovarian hyperæmia) in a very great measure, as I have repeatedly had occasion to observe." Yet again we are told a little further on that ovarian hyperæmia is sometimes met with as the result of marriage, and a state of things is induced which makes the unfortunate "sufferer shun the marital embrace." Now, we do not wish for one instant to suggest that Mr. Tait, or any other gynæcologist, would wilfully recommend a thoroughly unsound woman to marry on the chance that it might do her good; yet from the above sentences it seems that the probability of the marriage proving unfruitful is in Mr. Tait's eyes no sufficient ground for forbidding it. From the last sentence quoted above we see, too, what unfortunate consequences may result in addition to mere barrenness. A great teacher of medicine was in the habit of saying, that in ordering any powerful drug we must always weigh the probable good against the possible harm that may follow its use. In this case the possible harm may affect not only the patient, but the unfortunate husband who has been used in the light of a drug. A constantly ailing and probably irritable wife, a perpetually running doctor's bill, a neglected household, and the unsatisfied longing for children, are no small evils, and end more frequently in the Divorce Court than might be supposed from the light way in which some gynæcologists speak of marriage as a remedial agent. If the man is fully aware of the

[1] *The Pathology and Treatment of Diseases of the Ovaries. The Hastings Prize Essay of* 1873. By LAWSON TAIT, F.R.C.S. London, 1874.

condition of his intended wife, all responsibility of the medical attendant is at an end; but if, as is too often the case, he is kept in the dark until by bitter experience he finds that his wife is unable to perform the most important duty of married life, upon the medical man will rest the blame of all the misery such a union may lead to.

The greater part of the work is of course devoted to ovarian tumours and ovariotomy—and here Mr. Tait may wisely refrain from making a "mere summary of what is to be found in perfection in the recent publications of Spencer Wells, Attlee, Peaslee, and Gallez," and limit himself as far as possible to the results of his own experience. He divides what he has to say into diagnosis, treatment, and pathology. Under the first heading Mr. Tait ably points out the difficulties which surround the surgeon in the diagnosis of the more obscure cases. "There are perhaps," he says, "no diseases in the province of surgery where so much caution is necessary in weighing carefully every point in the history, every symptom, and every sign, for the purpose of establishing an accurate diagnosis. There are so many conditions which mimic them, and so few facts in connection with them upon which implicit reliance can be placed, that the only safety is to be found in the process of reasoning by exclusion; that is, for a proper diagnosis in the case of an ovarian tumour, it will be found the best plan, first of all, to make a mental list of all the conditions, that it might exclude them one after another, until no alternative is left." Even then doubts must occasionally remain, and in such cases Mr. Tait recommends delay, if possible, for the symptoms to manifest themselves more fully; or, if something must be done, a preliminary tapping may be had recourse to; or, in extreme cases, an exploratory incision. During the operation of ovariotomy Mr. Tait prefers the use of bichloride of methylene instead of chloroform, as he has not found it followed by the same vomiting and depression. In treating the pedicle he has made use of four plans, namely, "long and short ligatures, division by the écraseur, and inclusion in the clamp," and he has "arrived at the firm conviction that the clamp is the best in every way where it can be applied." The écraseur proved very successful in ten cases in which Mr. Tait made use of it, only one terminating fatally; but he conceived a distrust of it because in two cases pelvic hæmatoceles formed coincidently with menstruation a few weeks after the operation. In cases where the clamp cannot be applied he intends in future to tie the pedicle with silver wire, and drop it into the cavity of the abdomen, having found this plan extremely successful in two cases.

Mr. Tait recognises septic poisoning as an accident occurring with extreme readiness after ovariotomy, but he does not consider any special precautions are of much avail in its prevention beyond

pure air and freedom from hospital contagion, both in the place in which the operation is performed, and on the part of the operator himself. He has on some occasions used Lister's carbolic acid spray, but has not been able to recognise any beneficial effect from it. On the other hand his experience of it does not seem to be large, and may be said only to show that it can be safely used in cases where the abdominal cavity is open without the risk of carbolic acid poisoning or peritonitis. This being so, it seems to us to deserve a further trial even if its use is accompanied by some slight inconvenience to the operator. On referring to Mr. Spencer Wells's table of 500 cases of ovariotomy, we find that even in his hands "septic peritonitis," "septicæmia," "pyæmic fever," "hyperpyrexia" and peritonitis (fatal before the fifth day) are accountable for no less than 60 of 128 fatal cases, and it is possible that these numbers might have been diminished by the use of an antiseptic spray during the operation.

With regard to the pathology of ovarian tumours, Mr. Tait says he has found little up to the present time in the writings of others that harmonizes with his own observations. His experience and researches have led him to the conclusion that "all unilocular tumours in the neighbourhood of the ovary are not of ovarian, but of parovarian origin." In some cases the ovary may be stretched over the cyst-wall, or glued to it by adhesions, but careful examination in all cases has shown him a distinct separation between them. A similar conclusion has been arrived at independently by Dr. C. G. Bantock who read a paper embodying these views before the Obstetrical Society of London in April, 1873. In one case Mr. Tait successfully removed a large cyst of this kind without injuring either the Fallopian tube or the ovary. He records also a case in which, although the tumour was readily separable from the ovary and Fallopian tube, and was to all appearance parovarian in its origin, it was not unilocular, but composed of five or six sacs. There is, however, nothing surprising in this, for, as Mr. Tait says, if one parovarian tubule alone may become dropsical, there can be no reason why a number should not be so coincidently. Moreover, a similar tumour composed of two cysts was exhibited by Dr. Bantock at the Obstetrical Society in 1873, and he at the same time pointed out the fact that such cysts have a distinct peritoneal covering, which can be removed by a little careful dissection, carrying with it the ovary and the Fallopian tube, and leaving the cyst wall uninjured.

In one case of true ovarian cyst Mr. Tait has been able to confirm the observation first made by Rokitansky, and afterwards repeated by Dr. Ritchie, of the occasional presence of ova in small cysts. The case was one of small multilocular tumours of slow growth, affecting both ovaries, exactly similar to the cases previously recorded. This inclines the author to believe "that in these cases we have to deal with a special kind of ovarian tumour, occurring rarely and differing

from the ordinary adenoid growth." Rindfleisch seems to be of the same opinion, as he describes these tumours under the name hydrops folliculorum, and gives as their characteristics their small size, slow growth, and watery cyst contents. He does not seem himself, however, to have observed the presence of ova in the small cysts.

With regard to dermoid cysts Mr. Tait says, "the question of their origin lies between the hypothesis of an effort on the part of some over-active ovum in the direction of parthenogenesis" or "the equally hypothetical process of inclusion." He inclines strongly to believe in the former theory. The difficulties in accepting this explanation of the origin of these cysts, although made somewhat light of by Mr. Tait, is undoubtedly very great, as the recorded cases of true dentigerous cysts occurring in other parts of the body cannot be ignored. In the museum of St. Bartholomew's Hospital is a specimen of a tumour containing portions of skin and fat, serous fluid, and sebaceous matter, and two pieces of bone like parts of the upper jaw, in which seven well-formed teeth are imbedded. This tumour was found in the anterior mediastinum of a woman twenty-one years old (Paget's 'Lectures on Surgical Pathology'). A case has also been recorded of a dermoid tumour in the lung. We must remember also that, if Rindfleisch be correct, only three-fifths of the recorded cases have been found in the ovary. In fact, the whole subject is involved at present in obscurity, and we have scarcely sufficient data at present from which to form any definite theory. We must be content to accept the fact of their existence, and wait for further observations before venturing to theorize as to their origin.

Mr. Tait briefly concludes with a review of the opinions usually held as to the origin of the ordinary cystic tumours of the ovary.

Mr. Tait's views are original, and the essay will be read with pleasure by every one interested in ovarian pathology. It shows evident marks of the "years of study and personal investigation" which the author has devoted to the subject, and we hope he will continue his labours in the same direction.

X.—The Principles and Practice of Veterinary Medicine.[1]

It is a somewhat extraordinary fact that veterinary literature, in the form of standard works, in this country owes very little to the teachers of veterinary science in the various schools which have

[1] *The Principles and Practice of Veterinary Medicine.* By William Williams, M.R.C.V.S., etc. Edinburgh, 1874.

existed or now exist; and that a "writing" professor who has ventured to teach his science beyond the walls of the school to which he belongs, by publishing a book, is almost as great a novelty as a butterfly would be in Fleet Street. The few popular works on the treatment of the diseases of the domesticated animals which we possess, as well as those of a more scientific or technical character, and even some of the text-books used in the schools, have been the production of members of the profession who have not been "professors" in the ordinary acceptation of the term. Though it is about eighty years since the first veterinary school was commenced in England, and though other schools have been started, until now we have four, or, perhaps, five in existence (one in England, and three or four in Scotland), yet the works on veterinary medicine or other branches of this science, which have emanated from the pens of gentlemen who officiated, or now officiate, as teachers in these establishments, may be counted on the fingers, and the majority of these have been of little worth or are now obsolete. This is such an unusual circumstance in medical and, indeed, any other branch of science, that those who are unacquainted with the history and position of this somewhat important department of knowledge in Britain might be pardoned if they expressed themselves as astonished or even incredulous. And, perhaps, they would not be altogether satisfied with the explanation that it might be that the professors expended all their time and energy in inculcating the rudiments of their profession to students, whose attendance at college need not exceed twelve or thirteen months. After a young man has left the school at which he studied, he has little chance of knowing more of the subjects taught, and is utterly in the dark as to any advance that may take place in their scientific character. In fact, he remains at a standstill, so far as the professors are concerned, unless he pursues his studies by means of other assistance than he will obtain from his *alma mater*, and he may even retrograde unless he has a very tenacious memory, or has employed much of the few months he spent in attending lectures in taking copious notes.

Of the character of the teaching in the veterinary schools, as at present constituted, or of the capability of the teachers to teach, we have no means of arriving at anything like a just estimate. We only know that an extraordinary number of subjects, many of them very difficult, must (if advertisements prove trustworthy) be crammed into an extraordinarily brief period of study (less by at least one-half, or even two-thirds, than that deemed necessary on the Continent); and we are led to apprehend that the schools, as such, exist mainly for teaching only the elements of veterinary science: not for advancing it, nor enhancing its value in a scientific or utilitarian point of view; while the professors appear to have thought themselves bound to do little more than deliver a certain number of lectures of a

rudimentary kind, and commit their views on scientific subjects as little to paper as possible.

Professor Williams appears to be determined to form, in his own person, an honourable exception to the almost invariable rule which has hitherto prevailed, and by his efforts to redeem the reputation of veterinary teachers from the reproaches which have for so many years been heaped upon them, and which we have now repeated, though in no ill-natured spirit (for we must suppose they had their reasons, be these good or bad). In venturing upon this course, however, he has certainly exposed himself to be classed with those who, prompted by a desire to elevate their profession by imposing on themselves the laborious and oftentimes expensive task of adding to its literature, have been cynically designated "theorists," "bookmakers," &c., by many who have neither the education, energy, ability, nor honourable ambition to follow the same course. It is to be hoped that, now a breach has been made, other teachers who are competent may emulate his example, and do something to establish their profession on a progressive and scientific basis, and testify to the outside world that something beyond the mere tuition of young men is aspired to, and that a professor is cognisant of the other responsibilities and functions which pertain to his honourable and honoured position when he performs his duty to his students and his science.

In the pages of this journal for July, 1873, we gave a somewhat extended review of Mr. Williams' work on the 'Principles and Practice of Veterinary Surgery'; and we have now to notice a complementary treatise on veterinary medicine from his pen. This work follows the same plan as the other, and differs from it only in being a joint production: the assistance of other writers having been sought to prepare chapters on subjects with which Mr. Williams evidently did not feel himself capable of dealing in a sufficiently satisfactory manner, or which had only been previously treated in the French language. Both these works supply, to a certain extent, a want which has been felt for many years; as the only book which the veterinarian could refer to for information on the topics they embrace was Percivall's 'Hippopathology,' an excellent treatise in its way for the period in which it was written, but dealing only with the surgery and pathology of the horse, and now far behind the age.

As in his first work, so too in the volume now before us, the most prominent cause for complaint is the embarrassing and excessive amount of quotation from the writings of medical and other authorities whose opinions are often conflicting, some of comparatively little value with reference to veterinary medicine, and others again quite discarded, because proved to be erroneous by more recent and trustworthy observations. In a manual, such as this is evidently intended to be, this superfluity of quotation is not only a waste of

space which might have been devoted to subjects that are omitted, but must leave the reader or the student rather bewildered as to which opinion is to be received as preferable in Mr. Williams's opinion; for, in the majority of cases, he gives no opinion of his own. There are numberless facts in veterinary medicine which are perfectly established, and do not require to be quoted from other authorities, but may be looked upon as available for any writer who has occasion to deal with them, and they may be fairly incorporated into a treatise in the terms which best suit a compiler without in every instance acknowledging the authorities from whom they were last borrowed. It may look well-read, or show a desire to be impartial, to give the opinions of a number of writers in their own words, but when a man claims to be an authority one would prefer *his* opinion, and if not satisfied with it, then reference could be made to the works of other authorities.

And another matter, which is one more for regret than complaint, is, that through his inacquaintance with foreign literature, and especially with the excellent works on veterinary medicine which have been published in French, German, and Italian, Mr. Williams' book is greatly deficient in important information, and does not contain the results of recent observations and researches in comparative pathology. It does not, therefore, exactly represent the actual condition of veterinary science as it is known in Europe; though, perhaps, in some respects, it is quite abreast of it in this country, and in others again is not in harmony with the ideas and practice of the most successful veterinarians. Some subjects of no particular moment are diffusely treated, and others of the greatest importance are dismissed in a page or two. For instance, a chapter devoted to the modes by which death occurs occupies thirteen pages, while that allotted to etiology is dismissed in one page!

The chapters on contagious diseases should form, perhaps, the most interesting portion of the work, but for the reasons before mentioned they are very far from being compleie, and some of them are very meagre, especially those which deal with the more destructive scourges.

With regard to the origin of these maladies, we are favoured with the views of several medical and veterinary writers as to whether or not some of them at least may not be spontaneously developed; it is gratifying to find Mr. Williams here committing himself to an opinion without leaving us to wonder what he thinks of the question. He writes:

"Such are a few of the arguments that might be brought forward in support of non-spontaneity, and it must be confessed that they are very strong and difficult to overcome. Notwithstanding their force, and the high authorities from which they emanate, the author

is of opinion that it can be proved that glanders, rabies canina, the distemper in dogs, as well as the carbuncular diseases of horned cattle, which are propagated by inoculation, are spontaneously developed in this climate, the last-named diseases—the carbuncular—never originating otherwise, unless indeed there be an accidental or designed inoculation. I have arrived at this conclusion from a careful and extended course of observation, and after duly weighing the evidence upon both sides of the question. Mr. George Fleming, in his excellent book on 'Rabies,' already referred to, says: 'In certain maladies which develope a contagion capable of producing the same morbid disturbance that characterises them when transferred to healthy animals, we are almost, if not altogether, compelled by the force of reasoning and the power of indisputable facts, to admit their spontaneous origin from a concurrence of circumstances, many of them perhaps obscure, whose operation we are not always able to trace, save in the effects produced in the creatures subjected to their operation. Some virulent and well-known diseases in the lower animals must be placed in this category, and among them we need only now mention those vulgarly designated 'glanders,' 'farcy,' and 'strangles,' in the horse, 'anthrax' in cattle, the so-called 'typhus' in pigs, and 'distemper' in the dog. Numberless facts, indeed almost every day's veterinary experience, appear to demonstrate in the most unmistakable manner that these affections may be developed directly, without the intervention of any infecting medium.' Without any further quotation, I now purpose to relate very briefly some few facts which seem to be positive evidence in favour of the spontaneous origin of the above-named diseases. At the same time, it must be clearly understood that, with the exception of the carbuncular affections of horned cattle and sheep, these contagious diseases are but *rarely* developed, and never propagated otherwise than by infection and contagion.

"Several horses in a stud are simultaneously or successively attacked by some epizöotic disease, from which some die, the majority recover. One or two overcome the epizöotic, but remain debilitated, unthrifty, have a cough, lose condition; in fact, they hang fire, as it is commonly termed, the convalescence being prolonged and most unsatisfactory, one day seemingly doing well, the next day off their feed, shivering, with a staring coat, and a "tucked-up" flank. In the course of a period extending over a week or two, or as many months, a suspicious discharge appears at the nose, the submaxillary lymphatics become hard and swollen, the Schneiderian membrane assumes an unhealthy aspect—coppery or leaden in hue—and after a time the unmistakable ulceration of glanders is seen in the nostril, farcy buds appear on various parts of the body, and the animals are now unmistakably glandered and farcied. If kept in company with other horses the disease will be communicated to the healthy ones, but if the sufferer be isolated early, or destroyed, no further mischief accrues. From bad feeding, overwork, or ill-treatment, the disease termed diabetes insipidus is induced; the majority of the animals recover under proper treatment and a better *régime*.

Some of them remain subject to a return of the polyuria from very trivial causes, and finally it assumes an incurable character, remaining persistent in spite of tonics, good food, proper ventilation, drainage, exercise, &c. As the emaciation increases farcy buds appear on the surface of the body, suppurate, and discharge an unhealthy pus. By-and-by a discharge issues from the nose, and upon examination the horse is found both glandered and farcied; and, as in the other instance mentioned, a centre of contagion is thus formed, from which the disease spreads, if precautionary measures have not been taken.

"Glanders from contagion is always made manifest by a discharge from the nose, or by the formation of suppurating abscesses along the course of the lymphatics, without the advent of an interposing disease. Glanders produces glanders—and farcy is but a different manifestation of it—as small-pox produces small-pox, but the specific virus of glanders may be developed by the degradation of tissue consequent upon the ravages of another disease. Thus, in my own experience, in addition to the examples already brought forward, and which may be said to occur not very rarely, glanders has succeeded to a prolonged attack of rheumatism in a weakly-made, narrow-loined horse, to castration in the donkey; and it has repeatedly appeared in new and damp stables, amongst horses well cared for and humanely treated in every way, and where contagion or infection were quite out of the question, indeed impossible."

The development of glanders, independent of contagion, may be received as a fact, and the evidence in support of it is overwhelming. The disease in northern and western climates is an almost inevitable scourge of the equine species during war, and is as fatal in the open air as in the insalubrious dens designated "stables" in our large towns; in this respect it resembles human typhus and the typhus of camps. Mr. Williams statements are, therefore, quite in harmony with the experience of the best veterinary authorities, and are an addition to the mass of evidence which the believers in the non-spontaneity of contagious diseases will have to clear away before their doctrine can be received as correct.

With rabies, the spontaneous origin of which has been much debated in recent times, Mr. Williams is equally positive. "Rabies, like glanders, is occasionally found to originate spontaneously in this country, although many learned arguments have been brought forward to prove the contrary. The reader is referred to Mr. Fleming's work where he will find the subject most ably discussed. In support of the many instances brought forward by Mr. Fleming to prove this interesting point, I may state a circumstance which came under my own immediate notice some years ago. A watch-dog which was generally kept on the chain became rabid, and bit some cows and pigs on the farm before he was destroyed. In a few weeks afterwards the animals bitten became rabid—both pigs and cows. Now

it is a very easy thing for the supporters of the contagious origin of the malady to say, 'You have no proof to show us that the dog was not bitten by a rabid animal, and that he did become rabid from this cause.' It is certainly very difficult to disprove anything that might be asserted, but the facts of the case were as follow:—The farm was in a district of Wales where rabies had been unheard of for many years, indeed, where such a malady was almost unknown; the dog had been on the farm since his puppyhood, and was generally kept on the chain. It may be argued that these are negative facts. It is, however, positive that no rabid dog had been seen in the neighbourhood at least, and that the dog was not bitten by any dog, rabid or otherwise; and that the disease was rabies was abundantly proved by its development in the animals bitten."

The chapters on the contagious diseases of animals are perhaps the least satisfactory, and especially with regard to preventive measures. Some are very incomplete, and not up to date. For instance, speaking of cattle-plague Mr. Williams defines it as "a specific, malignant, and contagious fever, indigenous to the Asiatic steppes of Russia," &c. Now, researches into the geographical distribution of this terrible malady have for years established the fact that it prevails in as enzöotic a manner in Western and Southern China, Cochin China, Burmah, Bhootan, Thibet, various parts of Hindostan, and in the island of Ceylon, as it does in the Russian dominions; this is an important fact to remember, so far as our colonial empire is concerned.

Mr. Williams rightly points out an error in the statement of the Cattle-plague Commissioners, who, in alluding to the important fact that the blood contains the virus of the malady at an early period, as was demonstrated by Dr. Sanderson, assert that it is pregnant with consequences of much moment in medical doctrine, for though the existence of a similar fact had long been suspected in several human diseases, it had never been proved in any. "When the Commissioners made this assertion," says Mr. Williams, "they were evidently ignorant of the fact that Coleman had demonstrated the presence of the glanders poison in the blood, and had caused glanders by transfusion."

The more recent investigations into the pathological anatomy of cattle-plague are not alluded to, and there is an error in what is said with regard to the condition of the intestines: "True ulceration of the bowels is extremely rare, though its proper epithelial covering is easily removed." In some outbreaks ulceration of the intestines is very frequent, and diphtheritic exudations into their cavity are by no means uncommon. We have seen tubular masses several inches in length and some lines in thickness extruded from the anus during the lifetime of the animal, and after death have found the mucous membrane destroyed in large patches and the muscular tunic completely exposed.

The literature relating to malady is immense, and almost everything pertaining to it had been elucidated before it visited this country in 1865.

With regard to attempting to cure the disease, Mr. Williams says, "The cattle-plague may be classified as one of those diseases in which all methods of medical and hygienic treatment have hitherto proved unsuccessful, and, judging from the nature of the malady, will always prove unsuccessful." It is satisfactory to find that the author has arrived at this conclusion at last, as, if we remember aright, he was an ardent advocate for the establishment of sanitaria or lazarets for the accommodation and treatment of plague-stricken cattle in 1866. It is needless to say that had the Government assented to the institution of these, we might have had the malady among the herds now, or our cattle might have been utterly annihilated long before this.

The description of the contagious pleuro-pneumonia of cattle is fairly good. We note that, when describing the necroscopical appearances and particularly the alterations observed in the lungs, Mr. Williams quotes from 'Strangeways' Anatomy.' "In the ox and sheep, the subserous as well as the interlobular tissue is very thick, the lobules being distinctly visible. This distinctive conformation explains the speciality of inflammatory diseases of the lungs in those animals." This, and very much more pertaining to the structure of the horse, ox, sheep, and pig, really belongs to Chauveau's 'Comparative Anatomy of the Domesticated Animals,' the first edition of which was published many years before that of Strangeways. The statement in Chauveau's work is as follows:

"The lungs of the ox, sheep, and goat are remarkable for the distinctness with which the lobules are defined. They are, in fact, separated by thick layers of cellular tissue, continuous with the internal face of the visceral pleura. Dietrichs, who was the first to draw attention to this peculiarity in the larger ruminants, has justly remarked that it perfectly explains the altogether special characters of the lesions of pneumonia in these animals."[1]

We think it right to notice this fact, as on at least two occasions in this country almost literal translations of large portions of Chauveau's excellent manual have been borrowed without acknowledgment, and as an English edition appeared in 1873, this writer might have referred to the original source, in justice to the distinguished French physiologist and anatomist, if it was deemed necessary to quote any name in connection with this subject. When speaking of the prevention of contagious pleuro-pneumonia, Mr. Williams alludes to protective inoculation. This was proposed a

[1] We quote from the translated edition of Chauveau's 'Anatomy,' published by Messrs. Churchill in 1873.

number of years ago by Dr. Williams, of Hasselt, Belgium, who carefully studied the malady, and from discovering that an animal which had fairly recovered from its effects was refractory to a second attack, was led to institute a series of experiments in which he inoculated healthy cattle with the fluid drained from diseased lungs, and observed that they were no longer susceptible to the contagium. This inoculation was performed in various regions, but it was found that the virulent fluid was so active that in many cases serious consequences supervened when the part in which it was deposited was provided with much connective tissue. The distal extremity of the tail was therefore preferred, and it is now the part selected for the operation. As a protective measure, the benefits to be obtained from it were at first stoutly contested by many authorities on what might be termed theoretical grounds. One of these objections was that inoculation did not produce the lung-disease, but only a local effect of varying intensity; and that therefore it could not confer immunity. In no country in Europe, perhaps, has this notion been more prevalent and tenaciously held than in our own, and we might hazard the opinion that it is also believed in by Mr. Williams, for he writes: "When all legislative enactments have failed to prevent the importation and spread of the malady, it will be time for us to test the value of inoculation." This is surely strange language to employ in a matter of this kind, when the results of some well-conducted experiments, such as have been devised on the Continent, would demontrates whether inoculation confers any protection. Legislative enactments and sanitary rules are useless, unless carried out in the strictest and promptest manner by an efficient organisation. But then they may prove, in the embarrassment they occasion to trade and different industries, almost as bad as the disease itself; and when, as in this country, the organisation is of the most imperfect kind, and the measures are energetically carried into operation in one locality and not in another, nothing but failure and discontent can be the result. We have ample proof of this.

In other countries inoculation has proved a success, and the experience gained in them has quite borne out the conclusion of the Belgian Commission appointed to report upon Dr. Williams' protective measure: "The inoculation of the liquid from the lungs of an animal affected with pleuro-pneumonia exerts a preservative influence, and invests the economy of the larger number of animals subjected to its influence with an immunity which protects them from the contagion of this malady during a period which has yet to be determined."

Other Continental commissions have equally reported in its favour; inoculation is now largely resorted to in different countries, and in some in which this lung malady prevails it is even rendered compulsory, as in Holland and Australia. In the latter, what we

would consider the most convincing evidence of its efficacy has been furnished, and the highest veterinary authorities in Europe testify to its value. Surely this might be deemed sufficient to convince the most sceptical, and to lead them to advise a recourse to the operation, particularly in a country where imperfect sanitary measures cannot be enforced, because there are no efficient instruments to carry them into effect.

But even in a theoretical point of view, if we had not multitudes of practical facts to hand, there is very much in favour of this protective inoculation. The chief theoretical objection to it, viz., that it does not produce the particular lung lesions, is of comparatively little moment when we know that it causes local alterations which are identical in character, and are accompanied by more or less general disturbance: as increased temperature and fever, often a peculiar cough, and other symptoms. Vaccination, as a protective measure against human variola, does not induce the disease, but acts in a similar manner to this bovine inoculation; it would be of comparatively little value if small-pox were the result of the operation, and it is to be apprehended that if inoculation for lung-plague brought about the alterations in the pulmonary organs which are peculiar to the malady when it is induced in a natural manner, it would scarcely be justifiable to resort to it. A peculiar feature in this inoculation, besides the special character of the local lesions, and one which the opponents of the measure appear to be ignorant of, but which is a strong point in its favour, is the latent or incubative period that intervenes after the virulent fluid has been deposited beneath the skin, and the manifestation of the morbid phenomena that mark the advent of a successful operation. This interval varies from a week or ten days to as much as two months—the primary incision having nearly or quite healed up in the meantime. Nothing of this is observed in blood-poisoning or septicæmia, as the anti-inoculists would have us believe the effects of the virus really are; and, curiously enough, the period somewhat corresponds to the latent stage of the malady. The virus also possesses an extraordinary degree of virulency, the amount of citron-coloured exudation thrown out around the seat of inoculation being sometimes quite astonishing. In this respect, and in its pathological and histological features, it differs in no way from what is noted when the malady is confined to the lungs and the thoracic cavity. Not only this, but Reynal, director of the Alfort Veterinary School, an excellent and trustworthy observer, and one who is greatly in favour of the operation, states that the inoculated malady may even be contagious. An instance of this occurred in his own experience. Speaking of the symptoms that follow inoculation, he says: "C'est l'étude attentive de ces phénomènes qui donne la raison de l'efficacité de l'inoculation. Du reste, un fait que nous avons observé nous-même lève à cet égard tous les doutes. Une

génisse bretonne, inoculée par nous, a communique la péripneumonie à deux autres qui etaient placées à ses côtés dans une des écuries de l'École d'Alfort. Les lesions caractéristiques de la maladie ont été constatée, à l'autopsie, dans les poumons de ces deux génisses."[1]

There is no information of this kind in the work now under review, and this forms one of its greatest defects. Veterinary science is in a very rudimentary condition in Britain, and not nearly so advanced as on the Continent; so that to produce a book which would fairly represent the wonderful progress it has made within the last twenty or thirty years, and prove useful to those who do not understand foreign languages, or have not leisure to peruse the numerous treatises and journals issued abroad, reference must be made to the observations of the well-educated and thoroughly-trained veterinarians of Germany, France, Belgium, and Italy. The necessity of this is painfully evident in perusing the majority of the chapters in Mr. Williams' book, which contain no mention whatever of most interesting and valuable advances which have been made in our knowledge of the diseases of animals. His references with regard to these are chiefly taken from Professor Gamgee's works, and these are now almost out of date. These remarks apply more particularly to the contagious diseases, which are, after all, perhaps, the most important. In the chapter on glanders and farcy, for example, it is stated: "Glanders, and its variety farcy, originate spontaneously in the horse, ass, and mule, and are capable of transmission from them to man, and from man to the horse, ass, and mule." Nothing further appears to be known of its capability of affecting other creatures, though its eminently transmissible properties have been accidentally and experimentally proved times without number, and for a long series of years. Hamont, the director of the Veterinary School of Abouzabel, Egypt, has witnessed dogs and a lion in the menagerie of the Viceroy of Egypt, die from glanders through eating the flesh of a diseased horse. Saint-Bel and Spinola have reported the death of dogs, cats, and other animals from eating the blood, pus, or flesh of glandered horses; and a zoological garden in Germany for several years sustained a serious loss in its collection of carnivorous creatures, through feeding them on the uncooked flesh of solipeds which had suffered from this malady. Lafosse and Dulac saw a lion perish at Luchon, in 1864, from this cause; and towards the end of 1873, a splendid lion and tiger died of glanders in the zoological gardens at Berlin by infection. Leisering, of the Dresden Veterinary School, has seen glanders affecting a bear and other carnivora in the zoological gardens.

Bouley and Renault, Wirth, Hertwig, Polli, Decroix, Gerlach, Saint-Cyr, and others have successfully inoculated dogs with the

[1] 'Traité de la Police Sanitaire.' Paris, 1873, p. 458.

virus of acute and chronic glanders, and though the malady so induced was generally of a benignant character, yet when the virus was transferred from them to the horse or ass species, it regained all its destructive potency, and these succumbed to the malady in its most malignant form. Saint-Cyr's experiments proved that a dog once successfully inoculated was no longer susceptible to the influence of the contagion. Lafosse, of the Lyons Veterinary School, mentions the case of a dog which acquired the malady through cohabitation with affected horses.

Haubner has been successful in his inoculation of cats; and from the result of his experiments, he is of opinion that this animal is more readily infected than the dog, though, as with that animal, the malady is benignant.

So early as 1844, Wirth, of the Zurich Veterinary School, had proved that sheep and goats may be successfully inoculated, and he and Gerlach, as well as Renault, report the death of these creatures from the glanders so induced; thus demonstrating that the malady is not always benignant in the ovine and caprine species. The virus derived from them has always produced acute glanders in the horse. Gerlach asserts that the goat and sheep are more susceptible than the dog. Falke has, on two occasions, seen sheep affected with the disease through cohabitation; and Ercolani, veterinary professor at the University of Bologna, reports that a she-goat which was kept in a stable where five glandered horses had been lodged for fifteen months, was attacked by inflammation of the udder, and had an abscess in that gland; in ten days the creature was seized with general illness, discharge from the nostrils, and sniffling breathing. In ten more days it died; when glander-ulcers were found in the nostrils, pus in the submaxillary glands, and small tubercles in the lungs.

Hertwig, Gerlach, Renault, and Lafosse have only obtained traces of inflammation at the point of inoculation in cattle. Erdt, however, mentions the case of a cow which received glanders through cohabitation. Spinola, Gerlach, and Renault inoculated pigs. The first-named speaks of symptoms analogous to those of the chronic form of the disease being promptly induced, and as promptly subsiding spontaneously; Gerlach's attempts only resulted in the formation of an ulcer at the inoculated part, with swelling of the adjacent lymphatic glands; while Renault's experiments yielded only negative results.

Gerlach inoculated rabbits without producing any disease; but Röll, of the Vienna Veterinary Institute, asserts that at least a local malady of a chronic nature can be induced in them in this way; while Haubner and Schilling have obtained positive results.

Ercolani and Bassi have communicated glanders to mice; but Renault's attempts to infect fowls have never succeeded.

Though it is extremely probable that these facts are here made known for the first time in this country, yet it will be at once apparent to the hygienist and comparative pathologist that they are of very great importance, when it is remembered that this is a most loathsome and fatal disease in mankind, to whom it is readily transmissible. It is astonishing that a work which professes to deal with these maladies should not contain any allusion to the almost universal receptivity of animals for the virus of this equine and asinine affection.

In the chapter devoted to variola vaccina, when speaking of the pathology and symptoms of that disease, it is stated, "that the cow-pox and small-pox (human) are identical there can be no doubt; indeed, many experiments have been made for the purpose of demonstrating this." Notwithstanding such a positive opinion, there are still the gravest doubts as to the identity of the malady in the human and bovine species, and the result of recent experiments is strongly opposed to the statement made by Mr. Williams. For instance, a statement by Depaul at the Paris Academy of Medicine some years ago, induced Chauveau to undertake a number of experiments to test the correctness of such an assertion as that made by Mr. Williams. Numerous inoculations with the virus obtained from the pustules of human variola were made on cows, and nothing resulted but a kind of papular pustules having no analogy whatever with the true vaccine vesicle or pustule, and the lymph of these, when inoculated in man, always produced small-pox and not vaccinia. Repeated a great number of times, the same results were always obtained. Not only does direct experimentation negative the statement, but more than one history is to be found in the annals of medicine of the most untoward accidents occurring through believing in the identity of human and bovine variola. One at least of these happened in India some years ago, when a surgeon inoculated a cow with variolic lymph, and from the pseudo-vesicle produced he "vaccinated," as he imagined, a number of human beings, nearly all of whom were soon affected with small-pox and the majority of them died. Indeed, we find Mr. Williams himself gives us a case in point from the *Boston Medical Journal* for 1860, in which mention is made that during that year a Mr. Martin "inoculated some variolous matter, taken from a pock upon the body of a man who had died of small-pox, into a cow's udder, and subsequently vaccinated some fifty persons with the matter derived from the cow. Most of those so inoculated were attacked with variola, and three died." Surely this case alone might have been sufficient to prevent Mr. Williams from committing so serious a mistake as to lend his name to the perpetuation of a dangerous fallacy.

The author is no more fortunate with regard to the origin of horse-pox and its supposed identity with small-pox. He says, "It

is placed beyond a doubt that the horse is subject to a true equine pox of the same kind as the small-pox in man, and cow-pox in the cow. It is stated that the equine pox occurs as an epizöotic when small-pox is epidemic. When small-pox was raging in Edinburgh in 1872, I saw one case of what I considered genuine equine pox. The animal (a mare) presented signs of fever, had a slight cough, loss of appetite, with costive bowels for eight or ten days before any skin disease was observed. She was treated for a common cold, with fever; but at the end of the time mentioned small pimples were observed on various parts of her body, more particularly about the shoulders and back. At first these pimples were very numerous, small and pointed; the great majority of them, however, withered, and a small scab fell off, leaving the skin bare of hair. About a dozen on each side of the shoulders and upon the back increased in size, became vesicular, and afterwards pustular. These eruptions had very much of the character of those seen in cow-pox, namely, pimples increasing in size from a point to that of a horse bean, becoming vesicles in three or four days, and then pustules, depressed in their centres. In about ten days, or eighteen after the first manifestation of illness, dark, thick, solid scabs formed on the sites of the pustules. These scabs did not all fall off until fully a month had elapsed, and the hair was not completely restored in two months afterwards, when I lost sight of her. . . It has not been determined whether the small-pox poison originated in man, the cow, or the horse; whether man had the disease communicated to him from the lower animals, or whether horses and cows had it from man. The origin will most probably for ever remain a mystery, and we must be content with the knowledge that the disease is identical in them all."

What the somewhat extraordinary malady may have been which affected the horse in Edinburgh, it is certain that it could have no connection with the epidemic of small-pox; and from the symptoms enumerated, it is scarcely possible that the animal was suffering from horse-pox. The experiments of Chauveau have completely upset the notion that the two maladies are identical; indeed, the only animal to which human small-pox has been really transmitted is the pig. There is nothing in common between horse-pox and variola, except that they are both eruptive diseases which reciprocally exclude each other from an organism when they have once produced their effects on it. We may therefore concern ourselves but little with the problem as to whether man derived his variola from the cow or horse, or they theirs from the human species, seeing that the human malady is not transmissible to these creatures. A wide difference between the equine variola and that of nearly all other creatures, is its non-infectiousness; it being only extended by a "fixed" contagium. A description of horse-pox is one of the notable omissions

in Mr. Williams' book, the only other animal he describes as being affected with variola, besides the cow, being the sheep. We note that though the history of variola ovina has been pretty clearly traced, and is to be found in the veterinary literature of this country, Mr. Williams makes an almost unpardonable blunder in the statement that "sheep-pox was unknown in this country until 1847, when it broke out on a farm at Datchet." Careful researches have shown that the malady is really first mentioned for certain in Anglo-Saxon manuscripts of the ninth and tenth centuries, and which are now in the British Museum; that it is alluded to in Chaucer's 'Pardoner's Story;' and Mascal's 'treatise on the medicine of the domesticated animals,' published in England in 1596, well describes what the shepherds of his time called the "pocks"—the "poc addle" of the Saxon *Læce Bok.* Dr. Fuller in his 'Exanthematologia,' published in 1730, gives a graphic account of an outbreak on the South Downs, in Sussex, in 1710. The history of the disease on the Continent betrays a sad lack of knowledge. The symptoms are those described by Professor Simonds in his treatise on sheep-pox, and the chapter is meagre in the extreme. This is very unsatisfactory, as the malady is a very serious one, and its pathology is full of interest; nothing is said as to the recent investigations of Chauveau, Hallier, and Zurn into the histology of the vesicle or pustule; neither is any mention made of its communicability, nor of the most important and interesting experiments and observations of Röll, Haubner, Hertwig, Hering, Kersten, Gerlach, Gasparin, Dominick, Curds and Spinola.

The omission of the so-called "syphilitic disease" of horses in Mr. Williams' first work, and which we pointed out when reviewing it, is repaired in this volume by a translation from the French from the pen of Mr. Robinson, of Greenock.

The chapter on septic diseases, commonly designated "charbon" and "anthrax" on the Continent, is also a translation from the French by Mr. Anderson; while the succeeding chapter on the "Septic or Charbonous Diseases of Great Britain" is written, it would appear, by Mr. Williams. There was no necessity for this distinction and waste of time and space; as anthrax on the Continent is anthrax in England, and we cannot see that there is anything at all gained by describing it as it is seen in France and afterwards in this country. The peculiar equine malady erroneously designated "purpura hæmorrhagica" by veterinary authorities in this country, and which has been observed and studied on the Continent since the days of Laguérinière, is classed among the anthracoid diseases. This allotment of the affection may be disputed, as some of the most prominent features of septicæmia are absent, in the earlier stages at least. It is undoubtedly a specific affection depending upon an alteration in the blood, to which the term diastashæmia has been given by Delafond, and characterised by exudations and extrava-

sations, with rapid separation of the blood-globules and serum, the latter being reddish or citron-coloured from the commencement; but it is only when gangrene of some portion of the body occurs that the blood assumes the peculiarities noted in septicæmia.

What Mr. Williams designates "epizöotic cellulitis," "epizöotic pleuritis" and "pleuro-pneumonia" are merely season maladies. When speaking of the treatment of epizöotic pleurisy, and alluding to counter-irritation, Mr. Williams says:—"This is the favourite method of treatment at the present time, and it is accomplished, or thought to be accomplished, by the application of mustard, cantharides, or other irritants to the skin of the sides and breast. It is applied on the principle that no two inflammations can exist in the body at the same time; that an artificial inflammation of the skin, excited by the irritant, removes or destroys that which is going on within the chest. Others say that it rouses the capillary circulation, removes congestion, &c." For many years Mr. Williams followed this method, but at length abandoned it, and now exclaims against the practice. Nevertheless, we find him recommending external stimulants during the second stage of the disease, occurring in that condition when the horse neither gets better nor worse for several days. And on the next page we observe that warm water to the sides, followed by ammonia liniment, are prescribed. Also in canine "distemper" when pulmonary complications exist, hot flannel to the sides or hot fomentations are recommended; and counter-irritation is likewise counselled in ordinary pleuritis. Mr. Williams is wiser in his practice than his principles, and it is to be feared his recoveries from pleurisy would be few indeed, if he departed from the ordinary method of treatment which he deprecates, and abstained from counter-irritation. No veterinary surgeon who possesses common sense would think of applying cantharides blisters to the sides of a horse's chest nowadays; neither would he hesitate a moment in availing himself of the advantages to be derived from the judicious application of mustard and Mr. Williams' other counter-irritants to the sides when necessary, as in pleurisy.

The chapter on epizöotic cerebro-spinal meningitis is written by Mr. Lyman, of Boston, United States. We are informed that it is "a disease of which until recently comparatively little has been known. More especially is this the case in veterinary practice, and, indeed, I think it may be said that no veterinary writer has as yet given to it the attention which, considering its increasing frequency, it demands." The writer is evidently not aware that at the commencement of this century, Viborg, of the Copenhagen Veterinary School, had observed the malady prevailing for several years in an epizöotic form. At a later period it was carefully studied in Denmark by Stockfleth, and afterwards by Bagge; it has prevailed in that country almost every year since 1852. The writer of the

chapter adds: "The first appearance of this fever amongst horses that I am aware of in the United States was in New York during the winter and towards the spring of 1871, where it appeared with great virulence," &c. Lange gave us a description of the malady as it appeared in New York in1862, and we have before us an account of the same disease in that city, written by Liautard, and published in 1869. It was witnessed in France in 1863 and 1865. It has also been seen in Algeria, and during the French campaign in Mexico. It has been described as affecting the bovine species (though no mention of this is made in Mr. Williams' book) by Meyer in Germany in 1867, and afterwards by Jost and Anacker, who observed it in 1865 and the following years in Prussia and North Germany, and Utz and Vogel reported it from Wurtemburg and Baden in 1868. Bary has observed it in sheep, Mayer in goats, and Vogel in pigs. So far as the symptomatology and necroscopical appearances are concerned, the malady has been exhaustively studied by these various authorities.

With regard to the chapter on tuberculosis, Mr. Williams has been misled by the conflicting opinions which are held by many pathologists as to the formation and nature of tubercle. Thus he writes: "Tubercle was held by Laennec and others to be due to the formation of a non-inflammatory material originating spontaneously in the tissue; and by others that it was an exudation—a deposit of caseous matter from the blood. These views are now generally abandoned, and it is believed that the growth is inflammatory—that the cause of inflammation may be simultaneously the cause of tubercle,—that tubercle owes its origin to the absorption of the metamorphosed product of the inflammation. Thus Buhl was led to conclude that in the majority of cases of tuberculosis caseous matters existed in some part of the body, and that to the absorption of substances from these infected centres the general development of tubercle was owing. This seems to be a proper view to take of the origin of tubercle in horned cattle, for since the appearance of pleuro-pneumonia tubercular growths are much more frequently met with; but there is sufficient evidence to prove that cattle, more especially the high-bred ones, are constitutionally predisposed to wasting diseases, in the course of which tubercle and other allied formations become developed.

"We cannot, however, look upon tubercular diseases as they occur in horned cattle, as belonging to the same category as the phthisis pulmonalis of man—that is, an ulcerative lung disease—in its strictest sense. But similar conditions, both extrinsic and intrinsic, lead to the production and development of a chronic inflammatory disease in the lower animals, similar to, but not identical with, tuberculosis in man, and the development of tubercle is undoubtedly a local manifestation of a constitutional peculiarity.

"The experiments of Sanderson and others have proved that, in some animals at least, tubercle can be artificially induced by exciting a simple local inflammation of the skin and subcutaneous tissues. These throw much light upon the occurrence of tubercular growths as sequelæ to pleuro-pneumonia, and as it is proved that the exudate of a local inflammation in guinea-pigs tends to become caseous, it is fair to conclude that the same tendency exists in the lymph of pleuro-pneumonia, thus proving the conclusions of Buhl, 'that tubercle in the majority of cases was due to the absorption of caseous matter.'

"If the history of cattle, slaughtered sometimes in the primest condition, in which these (caseous) tumours are found could be traced, it would in all probability be discovered that at some prior period they had suffered from pleuro-pneumonia contagiosa, or other inflammatory disease. . . . I am of opinion that these excrescences are mere results of a previous inflammation, that the products of such inflammations are thus localised, whereby they are prevented from injuring the general economy; that in fact they are as much external to the general economy of the animal as so many excrescences upon the skin or tumours upon any part of the body," &c.

Mr. Williams is evidently unacquainted with the investigations of Schueppel and others, which have established an absolute identity between human tuberculosis and that of cattle—histologically and pathologically.

With regard to the relations as to cause and effect between contagious pleuro-pneumonia and tuberculosis, there is no proof whatever that any exist. On the contrary, there is every evidence against it. Pleuro-pneumonia is prevalent in some countries where tuberculosis is exceedingly rare, and the latter is very common where the contagious lung-disease has not yet been witnessed—as in Mexico, where about thirty-four per cent. of the cattle slaughtered for food are tuberculous.

With regard to inflammation of different organs or textures leading to tuberculosis, Mr. Williams appears to have yielded his judgment and observation to mere theoretical notions, for it may be safely affirmed that inflammation and suppuration do not seem to have much, if any, influence in the production of that condition. Tuberculosis is witnessed times without end in cattle which have never been known to suffer from any inflammatory disease, and whose lungs are perfectly healthy and exhibit not the faintest trace of previous disorder. And what is perhaps more to the point, is the fact that animals which are far more subject to inflammatory and pyogenic maladies are those in which tuberculosis is all but unknown—the horse, for instance.

No mention is made of those extensive experiments in tuberculosis referred to in this journal for October, 1874, and which, carried on for a number of years by various competent authorities, have established the fact that animals of widely different species, and naturally not at all disposed to the disease, can be infected in the most serious manner by ingesting tuberculous matter from people or animals, as well as by receiving into their stomach the flesh and milk of cattle which were victims of the disorder. Nothing is said as to its contagiousness, so far as we can discover.

Passing over the remaining chapters on sporadic diseases, which perhaps are of more interest to the veterinary surgeon than the general pathologist, we come to those on parasitic maladies, the principal chapter being written by Dr. Cobbold, who is well known as an able and enthusiastic helminthologist. His share of the work will, therefore, be read with profit. But, as in other portions of the book, we still have to complain of the almost total omission of what has been done by Continental veterinarians in this important department of medical science. The subject of parasitism has now for nearly a century largely occupied the attention of veterinary surgeons in different countries, and French, German, Italian, Danish, Swedish, and Dutch veterinary literature teems with their researches, and yet but little of this is known in England. With regard to two important subjects only—trichinosis and measles in the pig (*cysticercus cellulosus*) and ox (*cysticercus bovis*)—there is no mention of interesting experiments which have been conducted at some of the Continental veterinary schools, nor of observations published at various times with regard to these maladies. The same may be said with regard to nearly all the other parasites which infest animals.

Indeed the incompleteness of the work as a compilation is its most serious defect; and for this there can be no fair excuse, as in reality so abundant is the matter that may be selected from, that a diligent writer who could read in one or two foreign languages and was tolerably acquainted with his subject, would not find much difficulty in composing a work which would prove not only of inestimable value to the busy veterinary practitioner who is desirous of keeping himself *au courant* with the foremost workers in his profession, but might also be of much interest and assistance to those engaged more particularly in the study and cure of human maladies. Instead of a work written by several pens, and containing page after page of quotation from such works as Aitken's 'Science and Practice of Medicine' and other books which treat exclusively of human medicine, we would then be in possession of a treatise that would fitly represent veterinary science as it is now developed, and testify that it is

progressive and independent, and one well worthy the attention of cultivated minds.

A word as to the price of the volume—a matter of some moment nowadays. We think that for a book of only 704 pages, including the index, the sum of thirty shillings is excessive, and more particularly if we regard the manner in which it is got up, and remember that original writing and observation constitute but a very small proportion of the whole.

Bibliographical Record.

Mivart on Batrachia.[1]—Professor Mivart in the present work has given under a familiar title what is in effect an exhaustive monograph on the Batrachian order of Reptiles. Replete with curious and hitherto unknown information respecting the classification, habits, and mode of life of the chief families of the order, the study of the lowest group of air breathing vertebrates which in many respects show analogy to the highest groups, becomes in his hands of especial interest. We shall not follow the author through the general speculations which lead him to arrive at so many potent biological generalisations, but rather criticise the special points of the work.

He well remarks that "the exlporer of tropical lands and virgin forests has frequently to endure disappointment from the contrast between the richness of a known local fauna and the little to be actually seen of the animal population of the place."

Relegating therefore to the limbo of nursery fable the legend of the non-existence of the frog in Ireland, where in fact it is not only common, but numerous, he gives a minute description of the anatomy and development of the frog and toad. He collates the peculiar modifications of the female's back at the laying season in the *Pipa Americana* with analogous phenomenon observed in the Silurian fish, *Aspreas batrachus,* in which Dr. Günther has shown that the whole lower surface of the belly, thorax, throat, and even a portion of the pectoral fins showed numerous shallow round impressions to which a part of the ova still adhered. In this curious marsupial, or child-carrying fish, it is more than probable that towards the spawning time the skin of the lower parts becomes spongy, and that after having deposited the eggs, the female attaches them to it by merely lying over them. When the eggs are hatched the excrescences disappear, and the skin of the belly becomes smooth as before.

[1] *The Common Frog.* By St. George Mivart, F.R.S., Lecturer on Comparative Anatomy at St. Mary's Hospital, Professor of Biology at the English Catholic University. London, 1874.

Turning to the geographical distribution of Batrachia Professor Mivart remarks:

"We should be prepared for the fact that in South America tree frogs abound, since all kinds of animals in that region assume an arboreal habit. Monkeys are tree lovers all the world over, but nowhere are all the indigenous species so thoroughly arboreal as in tropical America. There alone do we find monkeys with a prehensile tail capable of serving as a fifth hand, and so affording a greater security and facility to locomotion among the branches. Only there also do we find beasts so exclusively constructed to pass the whole of their lives in trees that they can move along the ground only with difficulty. Such is the case with the sloths. Porcupines, which in the Old World have short tails, in the New World have long and prehensile ones. An animal allied to the badger—the kinkajoa (*Cercoleptes caudivolvulus*)—similarly acquires in South America a long and prehensile caudal appendage. Even the fowl and peacock order of birds becomes in South America more strictly arboreal than elsewhere (being represented by the curassows); and the very geese find there a congener (*Palamedea*) specially adapted to dwell in trees, and destitute (like the frog *Phyllo-medusa* before mentioned) of a web-like membrane between the foot."

Perhaps, however, the most remarkable and important fact, new to most English readers, which Professor Mivart brings to our notice relates to the genesis of that singular and marvellous batrachian which, under the name of axolotl (a word, by-the-bye pronounced easier than spelt, and whose fourth syllable by all the canons of Aztec pronunciation has the value of *e*), is to be seen in the Brighton and most other aquaria. Cuvier considered it to be a large eft-tadpole possessing permanent gills and gill-openings, with some other characters common to the eft-tadpole stage of existence. The succeeding generation of zoologists, however, thought it was a true adult, and their opinion was corroborated by the discovery that in captivity it possessed perfect powers of reproducing its kind. Slowly, however, the pendulum of scientific opinion has since swayed to the ideas of the Cuvierian age. For some years, individuals of this species have been preserved in the Jardin des Plantes at Paris, and a few years ago one individual, amongst others there kept, was observed, to the astonishment of its guardian, to have transformed itself into a creature of quite another genus—the genus *Amblystoma,* one rich in American species. Since then several other individuals have transformed themselves, but without affording any clue as to the conditions which determine this change—a change remarkable indeed, resulting as it does not merely in the loss of gills and the closing up of the gill-openings, but in great changes with respect to the skull, the dentition, and other important structures. There is, moreover, another and very singular fact

connected with this transformation. It is that no one of the individuals transformed (although we must suppose that by such transformation it has attained its highest development and perfection) has ever yet reproduced its kind, and this in spite of every effort made to promote reproduction by experiments as to diet, and as to putting together males and females, both transformed, also transformed males with females untransformed, and males untransformed with females transformed. Indeed, the sexual organs seem even to become atrophied in these transformed individuals. Moreover, all this time the untransformed individuals have gone on bringing forth young with the utmost fecundity, no care or trouble on the part of their guardians being required to effect it.

Professor Mivart plays this card as a strong argument in favour of his own theory of the "Genesis of Species." For here we have a rapid and extreme transformation, which takes place according to an internal law of the species which transforms itself. No one, moreover, has been able to detect the conditions which determine this transformation, though it takes place under the eyes and in the midst of the experiments of the observers. This fact appears, according to Mivart, to afford abundant evidence how obscure and recondite may be the conditions which determine the transformations of specific genesis, and how utterly futile are observations as to an apparent homogeneity of readily appreciable conditions. They are so since it seems to be just such recondite ones which really determine the changes just referred to, and probably, therefore, other changes analogous to them.

There are other genera of Urodelous Batrachian of which the genesis and development are equally uncertain. Thus it may be a question whether the genus *Menobranchus* may not also be a persistent larval form, and one which now never attains to its adult condition; if so, it is most probable that its lost state was similar to that of the exclusively American genus *Spelerpes*, the larva of which *Menobranchus* much resembles. With respect to *Proteus* and *Siren*, no conjecture of the kind can yet be made. Individuals belonging to the common English species (*Triton cristatus*) occasionally retain some of the external characters of immaturity in spite of having attained reproductive capability; and a European species (*Triton alpestris*) often matures the generative elements while still, as to external appearance, more or less in its tadpole stage of existence. The adult condition, however, is normally and generally attained by it.

Turning to the forms of *Batrachia*, of which the slow worms (*Cæcilia*) and the Amphisbænæ are the living representatives, Professor Mivart points out the close analogy between them and the extinct gigantic forms of Labyrinthodontia. In fact, the fossil form Ophiderpeton suggests to him the idea that the Ophiomorpha may

be the last remnants of a race which preceded and represented the subsequently developed serpents. Marine *Urodela* may have existed which bore the same relation to them that the great marine Ganoid fishes bear to the free existing Ganoids which are now restricted to the fresh waters.

One of the most singular characters of the marine turtles is the fact that they show bony lamellæ behind the orbit. The importance of this character might seem the more unquestionable, since no other reptiles and no birds or beasts whatever were known to exhibit a similar structure. Quite recently, however, Professor Alphonse Milne-Edwards has described a beast from Africa (*Lophiomys*) belonging to the Rodent order, which has a skull, the temporal fossa of which is similarly enclosed by bony plates. It is very well urged by Professor Mivart that this unexpected discovery destroys any weight which may be attached to this character as an evidence of true genetic affinity, *i. e.* of *descent.* It is doubtless inconceivable that such an abnormal Rodent could have directly descended through a common progenitor of frogs and turtles through a line of ancestors which never lost the cranial shield, though all the ancestors of all the other beasts, all birds and all reptiles except turtles, did lose it. And it is furthermore inconceivable that, if it were true, a variety of the lowest mammals, marsupials and monotremes could have less diverged from the ancient common stock than have the members of the Rodent order. Nevertheless, we have in these lowest mammals absolutely no trace whatever of a cranial shield. We have here, then, an undoubted example of the independent origin of structures so similar that at first sight their similarity might well have been deemed a conclusive evidence of affinity. We have thus a striking caution against hasty inferences from structural similarities. If this resemblance, as well as that of the dorsal shields, are taken together, no signs whatever of special genetic affinity, it is difficult to say what structural likenesses are to be deemed unquestionable evidences of a common ancestry. To our minds this argument (even allowing the freest scope for the operation of what has been called the law of atavism) is of the most potent nature. For it abrogates entirely the allegation on which Haeckel has so much insisted, that small structural resemblances (as *e. g.* the *augulus Woolneri*) are positive heraldic proofs of descent.

Another argument is much insisted on by Professor Mivart. Two of the ankle bones are elongated in a peculiar manner in the Batrachia. In the *Chirogaleus*, a small Lemurine of Madagascar, a similar condition exists. Now, it is absolutely impossible to believe that a special genetic affinity connects together, by a peculiarly common descent, half-apes and frogs. We are then driven to the conclusion that we have here again a striking similarity of structure in two instances which are quite independent in their origin.

That the power of rapid and prolonged "jumping" does not carry with it as a necessary consequence the elongation of ankle-bones, is demonstrated by the fact that in other animals which, to say the very least, jump no less than do these half-apes—as, for example, in the kangaroos, jumping shrews, and jerboas—it is not bones of the ankle, but bones of the foot proper, which take on an augmentation in length. Such considerations as these are followed up in the next chapter of the work which deals with the myology of Batrachia. One singular fact commented on by the author is, that the tongue in the frog and toad is so far different from the tongues of most familiar animals, that it is not free and movable in front, but behind, so that the Batrachian take their food by suddenly throwing forward, out of the mouth, the free hinder end of the tongue.

With regard to the nervous system of the Batrachia, Professor Mivart's researches are in direct opposition to those of Professor Ferrier. The fifth segment, the cerebellum, is very small, and then the same part in animals both higher and lower in the scale; indeed, in the frog class, this organ may be said to be at its minimum. When cut it exhibits no trace of an *arbor vitæ*. This fact has a special interest as bearing on alleged functions of this portion of the brain. It has been asserted by some that the cerebellum ministers to the sexual functions; by others that this part co-ordinates and directs locomotive movements; and quite lately that it is rèlated to movements of the eyes. The first two of these hypotheses seem to be completely overthrown by our frog. In the first matter there is anything but a deficiency of energy and activity; and, as to the second, many reptiles are less active and continuous than the frog in their locomotive efforts. As to the third hypothesis it should be remembered that the eyes of the frog are large and very movable, as also that they require a power of ready adjustment to enable the animal to secure its insect prey.

Our space precludes us from giving a more minute analysis of Professor Mivart's work. He has followed up the true monographical method of selecting an individual species, and his work may very well compare with those of Strauss-Durchheim on the Cat, Bojanus on the *Blatta Orientalis,* or Lyonnet "Sur la Chenille du Saulé." A safe and sure method of minute zoological teaching is thus pursued. With Professor Mivart's book in his hand, a student may well arrive at the comprehension of the complicated problems which surround the existence of the Frog as a solitary species, and may in time arrive at a state of knowledge which shall enable him with more or less probability to guess at the relationship or genesis of one of the most mysterious orders of cold-blooded vertebrata.

Quatrefages on Human Crania.—In January, 1874, we first noticed this important book, in course of publication in parts. Since that time the second *livraison* has appeared, and contains matter of at least equal importance to the first. It is occupied with a particular description of the second human fossil race, that of cromagnon, which was the chief population of the old Aquitanian bone caves. A great amount of real interest attaches to these bone caves of Aquitaine. It is true that they have given rise to exaggerated notions of Aquitanian civilization, but this circumstance will not, we hope, deter candid students to deny the antiquity of the race of human beings that inhabited them. The first undoubted example of these ancient people was found by Prof. Owen, in 1863, at Bruniquel, in the cave of Forges, associated with the remains of extinct animals. This discovery proved the existence, at the foot of the Pyrenees, at a period of time vastly distant, while as yet the mammoth ranged over the plains of Southern France, of man exhibiting features which it is impossible to separate from those presented by the existing Celtic or Celto-Roman races of the same locality. An attemdt has been made to associate some of these races with the Basques, who have certainly been hardly treated by speculative authropologists, and whose precise position in the series of races inhabiting Europe is rather less clear than in the days of Retzius. Formerly, we used to be told that the Basques presented cranial characters which, on the whole, resembled those of the Lapps, or Finns. The apparent resemblance which their language possesses to those tongues of a monosyllabic or Ugro-Tatar origin made some scientific men consider that the Basques must of necessity possess a skull whose cranial type should in the main accord with those of the short-headed races of Eastern Europe. But the large collection of skulls which Señor Gonzales Velasco collected in the graveyards of Guipuscoa led all candid students of Basque craniometry to admit the fact that there is really nothing peculiar in the Basque skull to distinguish it from the skulls of the Celtic races in its immediate vicinity, and it may be asserted that the characters of the skulls found in the Aquitanian caves have no further similarity to those of the Basques than to those of any other race. Another argument was brought forward in favour of the homogeneity of the populations in Southern France and in Esquimaux land. This theory was eloquently supported by Mr. Boyd Dawkins. The arguments in favour of the affinity of the old Aquitanian cave-dwellers with the Esquimaux do not appear to be of the strongest sort. "The habit of sculpturing animals on their implements" is common in all savage races; "the carelessness about the remains of their dead relatives" is also predicable of many; "the fact that the food consisted chiefly of reindeer" only proves that reindeer was an accessible

and plentiful food, and by no means denotes community of origin. The argument, put in a syllogistic form, ran thus: All who eat reindeer meat are "closely allied;" both Esquimaux and Aquitanians eat or ate reindeer meat; ergo, Esquimaux and Aquitanian cave-dwellers are "closely allied." We must deny the statement that the small stature of the Cromagnon men was "proved in the people of the Dordogne caverns by the small-handled dagger figured by MM. Lartet and Christy in the *Révue Archéologique.*" All who are acquainted with the small griped swords of the existing Hindoos, and in many of the so-called Phœnician sepultures, will know that they are held in the hand in a very different way to those of our own swords, and that the smallness of the gripe by no means connotes the size of the individual.

We cannot follow MM. de Quatrefages and Hamy through their enormously minute description of each separate skull from the Aquitaine deposits. It will be sufficient to observe that the type which to them is characteristic of the "reindeer man" is also found in the river valleys of the Seine. This type appears to differ entirely from the "tapinocephalic" skulls which Prof. Busk has so well described from English and Irish river valleys, and appears far more closely to resemble that of the existing Celtic of Europe. In fact, between such skulls as those of Mewslade, Borris, and Blackwater on the one hand, and those of Solutré on the other, there exists a wide gap, which seems to indicate a race difference, and that the Celts, if we may call them so, were not the earliest inhabitants of the British Isles. More it would be hazardous to infer until after the publication of Prof. Busk's 'Crania Typica,' which when at last it is published will, we are certain, throw a flood of light on the "priscan" crania of the early races (for there were several) which inhabited the British islands. A comparison of the Solutré skulls by *norma verticalis* with the Engis type shows that they did not essentially differ in any important details. It is true that some of the Solutré skulls are considered by M. Pruner Bey to present characters allied to the Esquimaux, from which, however, they are distinguished by the more bombate frontals and the less development of the zygomatic arches.

The Solutré skulls are thrown by our authors into two categories, those of the masculine and feminine types. This custom of classifying the male and female skulls from the same locality in separate lists is one highly convenient, and will render the present work of the greatest advantage of future students.

We imagine that the description of fossil man which is given in this *livraison* is almost at an end. The next part, which relates to the existing races of the world, will be far more interesting. For it will include descriptions of the dark races which are found distributed over the Indian archipelago. Mr. Andrew Murray some years ago

threw out a rough idea, which has been bizarrely paraphrased by some succeeding writers, that one and the same population of dark man inhabited early Egypt, the Dekhan, and Australia. This theory is perhaps a little too vague; but however this may be, there can be no doubt that such races as the Aïtas or negritos of Luçon, the inhabitants of the Andaman islands; of Rawak in the New Guinea archipelago, of the island of Tona in Torres Straits, of some of the Sandwich, Caroline, and Loyalty islands, bear a relationship to the Tasmanians of Van Diemen's Land, and to the Alfourons of New Guinea. It may be concluded that the affinity of the Veddahs of Ceylon, firstly to the existing Dravidian Cingalese, secondly to the Australians, cannot be predicated. The only trace of analogy appears to be with the aboriginal population of the Philippine islands, whose skulls are scarce in our public museums. It is with these that a detailed comparison, according to the hint thrown out by Professor Owen, must be made when we have sufficient material.

Certainly the present is the best, as it certainly is the largest and most costly, work on human skulls which has yet appeared in Europe.

Rett on Theory of Hallucination.[1]—This is a rather ambitious attempt to solve the many difficulties, and to throw light on the many obscurities connected with the subject of hallucination by means of a scientific menstruum. Although we are disposed to demur as to the success of the author, he has presented a clear and succinct account of existing opinion and of his own speculations upon the subject. After adopting the view of A. Comte that all our theories and knowledge may be relegated to three periods or stages of mental progress, the theological, the metaphysical, and the positive; and after analysing that author's views so far as they can be brought to bear on the matter viewed from the "positive" side, he classifies hallucinations into those which localise false perceptions in the periphery of the nervous system; those which associate this symptom with morbid irritation of the cortical substance of the brain; and into those which connect them with parts intermediate between the organs of the external senses and those subservient to perception. He also gives a copious catena of the theories of modern pathologists and psychologists. He disposes of the hypothesis of Müller and others of his school that hallucinations were due to morbid changes in the retina, auditory nerve, &c., by what has always appeared to us the crucial objection of Baillarger, that the character or complexion of the hallucination is altogether independent of the condition, healthy or unhealthy, of these tissues, and are determined by the prevailing morbid emotions; and are

[1] *Théorie Physiologique de l'Hallucination.* Par le Dr. ANT. RETT, Ex-interne de l'Asile des Aliénes de Fains (Meuse). Paris, 1874.

accordingly beautiful or hideous, inspiring hope or fear in relation to the presence of melancholia, monomania of gaiety, &c. This catalogue of theorists includes, first, Gall, who assigned to particular convolutions of the hemispheres the recognition of the supernatural, the marvellous, and credence in impressions of these kinds whether true or false; second, Foville, who referred the origin or hallucinations to lesions of those portions of the nervous system which connect the organs of the external senses with those of perception, or to the origins of the nerves of special sensation; third, Dagonet, who regards hallucinations as the result of erethism or hyperæsthesia, of those parts of the encephalon, and especially the walls of the ventricles in which the nerves of special sensation originate; fourth, Kahlbaum, who admits two species of hallucination; one, centrifugal, associated with the cortical substance and radiating towards the periphery; another, centripetal, having their seat in organs behind the roots of the nerves, the morbid excitement of which imparts to them a psychical and objective character; fifth, Audiffrent, who refers hallucination to the condition of the sensitive ganglia of the brain; sixth, Moritz Schiff, upon whose experiments (in which different parts of the brain were subjected to the influence of heat and galvanism, the influence being disseminated to the periphery tracts), is founded the conception that while impressions of vision, hearing, &c., are connected with different regions of the optic tract, general sensibility has no distinct localisation, but is associated with all the regions of the cortical periphery. M. Rett supports views he puts forward by evidence derived from comparative and pathological anatomy and from experimental physiology. Space will not allow us to enumerate, even to analyse the facts relied upon, nor to allude to any except the experiments of M. Fourné, who injects caustic coloured solutions into various parts of the base of the brain, having the effect of hardening these without injury to the adjacent organs, and where the results seem to establish a connection between the abolition of vision, hearing, &c., and the destruction of certain given points in the optic tract. M. Rett awards the merit, if merit there be, of his discoveries to the teaching of M. Luys; but, while we avoid any declaration of faith in the supposed discovery, we protest against any proposition which is supported by or necessitates such reasoning as the following: "Can we compare the mass of cells which constitute the cortical layers to a true photographic negative upon which the representation of all impressions, internal and external, condense themselves?" or, "Such impressions having once reached the interior of the grey matter of the optic tract, divested of their original sensorial character, pass into a more advanced stage of arrangement and development, and, metamorphosing themselves into a new form, become perceptible" (p. 32). We

shall now, in conclusion, state the author's theory, allowing him to speak very much for himself: First, (1.) hallucination is a morbid symptom, the anatomical substratum of which is the ganglionic cells of the centres of the optic tracts. The normal dynamic character of these cells is the reception and elaboration of exterior impressions, and may by pathological causes be placed in automatic activity. The products of this supposed irritation radiate through the white fibres to the cells of the cortical substance, which placed in vibration (*sic*) produce an indefinite series of morbid perceptions in proportion to the condition of the said cells. (2.) The automatic irritation of the ganglionic cells of the anterior centre of the optic tract radiating into the intellectual cells (*sic*) of the brain produces hallucinations of smell; B, of those of the proximate centre, hallucinations of vision; C, those of the median, those of general sensibility; D, those of the posterior centre, hallucinations of hearing, and those of the grey matter, hallucinations of cœnesthesis. (3.) Hallucination is not a process of new formation, but a sensorial perturbation of those parts destined to receive external impressions.

The existence of hallucinations having a psychical origin is denied or identified with delirium, but the theory is supposed to elucidate the presence of hallucination after the destruction of the organs of the external senses or independently of these; the phenomena of unilateral hallucination and the contemporaneous association of hallucinations of the different senses with errors of the cœnesthesis (p. 75).

I. **Darde and Burlureaux on General Paresis.**[1]—Making allowance for the inevitable difficulty in distinguishing delirium in acts from delirium in the thoughts and emotions which induce such acts, a difficulty which the author has often obviously encountered and been compelled to disregard, M. Darde has produced a creditable monograph upon the limited thesis to which he has restricted his observations. Many of his conclusions are worthy of further illustration, many others are either premature or open to grave doubts. To the first class belong his remarks upon the prolonged existence of prodromes, such as impairment of continuous memory, of intelligence, and of the capacity for ordinary occupations before any alteration in motility, and before any suspicion of the real nature of the impending disease; B, secondly, the delay in medical or legal interference until the occurrence of some terrible catastrophe, or of some sudden and inexplicable perversion of character; C, the non-appearance of cases of this disease in asylums, or under private

[1] 1. *Du Délire des Actes dans la Paralysie Générale avec Observations recueillies au Bureau Central d'Admission de Sainte Anne.* Par le Dr. FERDINAND DARDE. Paris, 1874.

2. *Considérations sur le Siége, la Nature, les Causes de la Folie Paralytique.* Par le Dr. CHARLES BURLUREAUX. Paris, 1874.

treatment, until the initiatory stages have passed; D, his opposition to Calmeil's opinion that a course of low dissipation, &c., are not symptoms of the malady, although they affect the patient; E, the frequent development of general paralysis shortly after marriage, and F, the long duration of the malady. Under the second class may be arranged, *a.* The rigid distinction of general paralysis into the expansive and depressive forms. No one can dispute that general paralysis may follow melancholia and other mental affections, or that gloom and darkness may overcloud the brightest visions of a patient, as similar shadows may cross the brightest aspirations of the sane, or that sorrow and taciturnity may be associated with or even supplant the exhilaration and delirious ambition of a paralytic; but such an admission does not justify the recognition of a new species of the mania of exaltation. *b.* The attempt to establish theft as characteristic of the expansive form of general paralysis, and homicide and suicide as characteristic of the dementia of the depressive form. That pilfering, purloining, and various modifications of dishonesty may be associated with general paralysis is known to every alienist; indeed, we have seen very recently, 1873, a paper by Dr. Burman in the 'Journal of Mental Science,' on larceny as committed by patients in the earlier stages of general paralysis; but it is equally well known that similar acts are committed in other species of insanity, and that even when observed in general paralysis they should be regarded as expressions of the predominating delusions as to wealth, power, and universal proprietorship rather than as diagnostic marks of a secondary kleptomania. In like manner, murder and self-destruction may be perpetrated during dementia, following any form of mental disease, even during idiocy, and we have even seen assaults on the part of general paralytics when their pretensions were gainsayed or opposed, but that such can be demonstrated as of frequent occurrence, or as involving any pathological significance, we cannot admit. Even the cases of suicide adduced by the author do not support his view, as they generally refer to mere attempts to what may be called conservative self-immolation, involving their own frustration, and suggesting what has been called experimental suicide, where measures for self-preservation precede or accompany those apparently taken for self-destruction. Neither Brierre de Boismont, Falret, nor Forbes Winslow, so far as we have examined their works upon this point, give instances of such a combination, and our own experience is equally barren.

The main object of the work is to show that delirious acts differentiate general paralysis, and is supported by a long array of cases, the principal features of which are as follows: 1. Extravagance, absurd purchases, motiveless weeping, repetition and omission of words in writing. 2. Breach of trust, pawning of wife's jewels, purchase of useless articles, but actual condition not detected until

occurrence of defective articulation, alternate excitement, and depression. 3. Sale of valuable in order to purchase valueless objects, imprisonment for purchase of stolen goods. 4. Lavish beneficence, intact memory, defective speech, feeling of weight in limbs. 5. Watchmaker mistakes copper for gold, cuts up bedstead in search for gold. 6. Exaggerated penitence, incendiarism, hesitation in speech. 7. Vociferations, alcoholism, profuse alms. 8. Wild projects, construction of fortifications of iron, of a cannon to shoot 100 or 1000 people at once. 9. Debauchery, craving for carriage travelling, swallowing lucifers, momentary hesitancy. 10. Theft, perverted conscience, abstraction in public, intended appropriation of barrel of wine by aid of the police. In many of the examples given the morbid mental manifestations constitutes the sole or chief, and in all an important element in the diagnosis of the disease.

II. Although the recapitulation of obsolete or discarded or dismembered opinions on pathology somewhat resembles a field of battle, or what Sir Walter Scott calls "the relics of a former world," such *résumés* are sometimes useful as showing the materials upon which prevalent theories are founded, or of which they are made up, and as warning us against former errors and failures; and we therefore conceive that M. Burlureaux has acted wisely in introducing his own views by a sketch of the views of his predecessors. We propose to present this sketch to our readers, which it will be observed is not chronologically arranged. 1. M. Magnan holds that the lesions in general paralysis pervade both the white and cortical substance, and penetrate equally from the centre to the circumference, and from the circumference to the centre. 2. M. Luys argues that it is unphilosophical to regard this disease as diffused generally through the various organs associated in the encephalon, and localises it in the cerebellum. 3. M. Bouillaud having recognised (1846), a connection between the disorders of locomotion in paralytics and alterations in the cerebellum, likewise regards this organ as the seat of general paralysis. 4. Calmeil, while admitting that the cerebellum may be involved in the changes consequent upon general paralysis, or, as he subsequently (1859) designates the affection diffuse chronic periencephalitus, has not abandoned the opinion promulgated in his original monograph, 1826, that the seat of the disease is to be found in the peripheral parts of the brain and membranes. 5. Bayle holds that the origin of the disease can invariably be traced to the arachnoid membrane, that its progress is from this structure to deeper seated tissues, and that it is to be defined as a true chronic arachnitis or meningitis. 6. This view is combated by Westphall and others, who have attempted to prove that the lesions may proceed from within, outwards, or from the brain to the meninges. 7. Baillarger and Delange believe that the induration of the white substance may be invariably observed in general paralysis.

8. Parchappe localises the malady in the middle layer of the cortical substance. 9. Joire, following in the steps of Virchow, Rokitansky, &c., conceives that granulations and effusions in the ventricular cavities, especially the fourth, form the most important lesions in general paralysis. 10. MM. Jaccoud and Magnan assert that serosity in the ventricles is to be found in other forms of mental disease, and especially in those where there are vertigo, obtuseness of external senses, feebleness of intellect and memory, impairment of speech, and general paralysis. 11. Franz Meschede has found associated with this disease, but especially with the stage of dementia, swelling and softening of the interior layers of the cortical substances, with hyperæmia and pigmentary coloration, and a fatty degeneration of the cells. These changes, as well as capillary apoplexy, have been discovered by the microscope, but are detectable by the eye. 12. Lockhart Clarke has signalised pigmentation of the cortical substance and simultaneous changes in the medulla as present in general paralysis. 13. M. Salamon, a Swedish physician, accepting the cortical substance as the seat of general paralysis, conceives that he has demonstrated that the actual source of the symptoms is explained by the compression or strangulation of the cells by the connective tissues. The existence of this condition has not been confirmed by other observers. 14. MM. Poincaré and Bonnet de Nancy attribute to diminution in the number and to alterations in the structure of the ganglionic cells of the lymphatics, and to the consequent disturbance in the vaso-motor influence, all the phenomena of the disease. 15. M. L. Meyer, confining his attention to the initiatory stages of the disease, describes the production of a cellular tissue surrounding the capillary extremities of the arteries and veins, altogether independent of the walls of the vessels, but which forms a complete sheath around them, as the cause of the functional changes. M. Burlureaux's own conclusions upon this much agitated question, which he rests greatly upon the researches of M. Voisin, but which are supported by many arguments derived from the study of symptoms, of the effects of remedies, &c., are as follows: *a.* The seat of general paralysis is the periphery of the encephalon. *b.* The character of the malady is inflammatory. *c.* All the causes which produce inflammation of the periphery of the encephalon likewise produce general paralysis.

Chicago Journal of Nervous and Mental Disease.[1]—We hail the advent of this new fellow-labourer and competitor in the field of periodical literature from the far West. It issues from Chicago, that Phœnix of cities, which seems to rise greater and stronger from the ashes of each successive conflagration. It is a

[1] *The Chicago Journal of Nervous and Mental Disease.* Nos. 2 and 3, April and July, 1874. Edited by Professor Jewel and Dr. Bannister.

valuable contribution from a region hitherto unknown in the geography of literature, and lying even in the United States, remote from what are regarded as the centres of knowledge and intelligence, being distant from Boston 800 miles, and from Washington 600. Whether the serial is intended to supply a desideratum in a department of scientific inquiry ably represented by the 'American Journal of Insanity,' or to meet a call created by the development and spread of mental and nervous diseases among our hyperenergetic relations, we cannot affirm; but it must be regarded as an important indication of the activity and of the abundant resources of the American medical mind. The numbers now before us consist of about 300 well and closely printed pages, divided into sections devoted to original articles, &c., reviews and bibliographical notices, an editorial department, and a periscop. Unavoidably, much of the matter, even in the first section, consists of translations and selections, but these appear to have been made with care and judgment, and to be strictly within the scope of the periodical. Among the most interesting articles are those upon the "Pathology of the Vaso-motor System," from the pen of Professor Jewel, one of the editors, and upon "Speech as a Reflex Act," by Dr. Onimus. In the former the author argues, clearly and cogently, though we do not apprehend convincingly, in favour of the existence of vaso-motor centres scattered along the substance of the spinal cord for different parts of the trunk—perhaps a continuous column of such centres, as M. Luys has supposed. As to the mode in which these centres diffuse influence to the various organs over which they preside, Dr. Jewel expresses himself as follows, "Each muscular vessel, or small vascular territory, has its ganglion, or group of ganglion cells, on which it is directly dependent. This nervous apparatus is quite sufficient for ordinary purposes but it needs control, and in view of thousands of similar mechanisms, coordination, so as to maintain an equilibrium of action in the whole body. This must be done by ganglia that have a wider sweep of relations, and as you retreat from the remoter parts of the body towards the centre, you find such ganglia larger in mass, and more extensive in connections—representative ganglia. Such ganglia we find in the chain of these bodies, on either side of the spinal column, and in the semilunar ganglia of the solar plexus. For the purposes of vegetative life this is sufficient." We quote this rather hypothetical opinion, not for the purposes of criticism, but in order to show the parallelism between the current of thought prevailing among physiologists of the United States and of the Old Continent. We find an exhaustive exposition of injuries to nerves and their consequences, in an analysis of several works bearing directly or indirectly upon the subject, but especially of that of Dr. Weir Mitchell, which has been translated into French, with an introduction written by Professor Vulpian. Dr. Mitchell enjoyed unrivalled opportunities for observa-

tion, as, during the War of Secession, a special military hospital for nervous affections was confided to him, to which were sent, from different parts of the theatre of war, the wounded who suffered from lesions of the nervous system; but the paper under consideration consists nearly as much of a statement of the views and inferences of Professor Vulpian as of those of Dr. W. Mitchell. As it is not long since we presented our readers with a review of Dr. W. Mitchell's work, further notice of the article in question is uncalled for.

We can recommend for perusal, "Remarks on the Inhibitory or Reflex Paralysis," "A Case of Chorea treated by the Exclusion of Light," &c.; but must direct special attention to the following condensed extract from the proceedings of the Academy of Medicine of Cincinnati, published in the 'Enquirer' newspaper of that city. Dr. Bartholow describes experiments made by him on the brain of a woman in the Good Samaritan Hospital. The woman, æt. 32, affected with an incurable disease, was almost *in articulo mortis* during the three weeks that the experiments continued. A cancerous disease, occupying the cicatrix of a burn on the back part of the head, received in childhood, brought her to the hospital. A large portion of both hemispheres of the brain was exposed in consequence of the removal of the skull by cancer. On the ground that portions of the brain had been removed under other circumstances innocuously, Dr. Bartholow introduced extremely fine needles into the structure for the purpose of electrical experiment. These needles were introduced ten times in each hemisphere, at various points, and to a depth of from a twelfth part of an inch to an inch and a half, and, as they were insulated to near their points, a Faradic current could be directed upon a small extent of brain substance. The patient, being in entire possession of all her faculties, remained altogether unconscious when the instruments passed through the dura mater and penetrated the brain. When the needle passed to a certain depth its presence was known to the patient by pain and tingling in the arm and hand on the side opposite to that where the puncture was made. When the needles were connected with the galvanic battery distinct muscular movements took place, with more intense pain in the extremities. Choreic movements, such as nodding the head and motions of the hands, were likewise noted. What is called unilateral epilepsy was produced by augmenting the strength of the battery. A post-mortem examination showed that the brain substance had been destroyed throughout the course of the needles.

In No. ii the following conclusions drawn by Dr. Bartholow are quoted from the 'Archives of Electrology and Neurology,' for May. The parietal parts of the brain have to do both with motion and sensation.

As regards motion, they are chiefly associated with the extensor

muscles (compare Ferrier's observations on electric excitation of the tubercula quadrigemina). An irritative or mechanical lesion of these parts will cause crossed unilateral choreic movements. Electrical excitation causes crossed muscular movements in the extensor muscles. Stronger electrical excitation gives rise to unilateral epileptiform convulsions on the opposite side. Lesions, merely irritative, mechanical or electrical, give rise to pain on the opposite side of the body, more especially in the hands and forearms. Pages 375-6.

We have adverted to this case both because of its unique and intrinsic character, and of the interest and indignation created in the public, and, if we may judge from recent occurrences during the meeting of the British Medical Association at Norwich, and in the subsequent proceedings before the magistrates in that city, likewise in the professional mind, by the subject of vivisection. "Since sin entered the world, and death by sin," there has been a craving, sometimes an unholy craving, to pry into the issues of life and death, to dive into the very fountains of health and strength, and to solve the problems of our being by gazing on growth, disease, decay, in their very act and operation. The evisceration of criminals and the observations of wounds and mutilations in man and animals satisfied curiosity, for it was little more, in early times; but it remained for our age to recoil from the horrors attending Majendie's experiments, we do not speak of their results; and to shudder at the vividissection practised at the Veterinary School of Alfort, where horses were for weeks, or so long as life continued, subjected to flaying, the removal of successive layers of muscle, the ligature of arteries, and various modifications of experimental surgery. It was predicted, when Professor Ferrier's experiments on the Quadrumana were made public, that a short step would lead to their repetition on man. This prediction has been fulfilled, and we have now a narrative of a living, apparently dying, but conscious human being subjected to processes which were certainly not calculated to prolong life, to assuage suffering, or to secure that repose and peacefulness which are so desirable and so much desired during our last hours. It is understood that Dr. Bartholow has made such reparation as excuses and apologies could afford, although it is somewhat difficult to understand whether this atonement be made to the departed or to outraged humanity, and we would not record one sensational or condemnatory word; but we do conceive that, before further rash, it may be enthusiastic, steps be taken in the same direction, that some competent tribunal or authority should sum up the benefits which are supposed to have accrued to science or to mankind from the destruction of life and the infliction of pain, and should decide whether, and how far, such measures are necessary or justifiable.

Dr. Jewel continues his paper in No. iii on the pathology of

the vaso-motor nervous system, but directs attention chiefly to the phenomena of active congestion. This he defines to be congestions occurring suddenly, and which do not depend, so far as we can see, on any physical obstruction whatever, to the progress of the blood current, while the vessels are still open and the way free, or even more free or open than is the case in the natural state, and cites the act of blushing deeply, the congestion of the mucous membrane of the nose, throat, larynx, of the bronchial mucous membrane, or those which happen occasionally in secreting glands or those seen in many affections of the skin, as examples of this condition. Congestion is an important, and perhaps the initial factor in inflammation, but it does not constitute the whole of the process. The characteristic features of this factor are described as the backward and forward or oscillatory motions of the blood in the vessels, and temporary quickening of the stream in vessels which are *above their normal size,* and a brief cessation and subsequent resumption of all such movements.

The following causes are given as those enumerated by authors: 1. An abnormal attraction between the tissues and the blood, by which the fluid is, in a measure, *sucked* into the fixed tissue. 2. Physical obstruction to the blood current, such as external passive constriction of the vessel, internal embolism or thrombosis, by which the passage is plugged. 3. Paralysis and consequent dilatation of the small vessels. 4. Loss or perversion of the propulsive or peristaltic power of the small muscular vessels.

A very large portion of the paper is devoted to the discussion of the various proofs, indirect and direct, in support of the latter theory which has been adopted by the author. These are derived from the consideration of the anatomical relations and physiology of the circulatory system, from observations on the action of the organization of the lower forms of animals from the effects of mechanical or animal irritants, &c., the majority of the illustrations being borrowed from the thesis of M. Legros' 'Des Nerfs Vaso-moteurs.'

There is likewise an original article of some interest on a case of facial neuralgia treated by repeated sections of the branches of the trigeminal nerve; but a large proportion of the contents consists of translations and selections. Many of these contain important and valuable information, but our space will not permit of more than one specimen which we regard as opportune at a time when so much attention is bestowed upon the localization of the functions and upon the unification of the different parts of the brain; as showing with what an imperfect and rudimentary organ mentalization &c., may be carried on.

Malinverni Germano, Prof. of Path. Anat. of the University of Turin, describes a case of a man in which there was found, after death, *a complete want of the corpus callosum, of the septum lucidum, and of the gyrus fornicatus, or convolution of the corpus callosum without*

noticeable impairment of the mental faculties or disorder of sensibility or mobility. He was a countryman, had always enjoyed good health, and passed as usual his term of military service, and finally died, aged forty years, with a form of gastro-enteric disease, at the great hospital of Turin. In other respects the brain was in a normal condition. Prof. Germano says, "In Physiology it is commonly understood that the corpus callosum serves to connect, co-ordinate, to harmonise, to unify or sum up the operations of the two cerebral hemispheres, to serve as the anatomical medium of their actions. But if the action of the cerebral hemispheres, as was observed in this case, may be regularly carried on, apparently in co-ordination, in spite of the absence of the corpus callosum, what other part in the cerebrum has in this case supplied the place of the great cerebral commissure?"

Prof. Germano regards this as an instance of arrest of development at probably the fourth month of intra-uterine life, and looks upon the brain in the case he describes as comparable with that of oviparous vertebrates, in the brains of which animals the corpus callosum is normally wanting (p. 377).

We regard the 'Chicago Journal' as a valuable addition to the annals of medicine.

Clinical Record of the Pellegrini Hospital.[1]—This volume is the second portion of the report of the Pellegrini Hospital, Naples, for the year 1872, the first portion of which we noticed in April last. The general arrangement is the same as in the preceding volume, and whatever faults of detail we might be disposed to point out, there can be no doubt that, as a whole, the work is extremely creditable to the hospital from which it emanates. It indicates that a great deal of good work has been done, that it has been carefully noted from day to day, and that the records have been cast into a shape which makes them available for general reference.

The volume now under consideration embraces three important subjects—wounds, scalds, and fractures; but by an apparent oversight the wounds and scalds belong to the third quarter of the year, while the account of fractures, with which the volume concludes, refers to the second. We shall speak of each of these sections in order.

The wounds are classified according as they affect the head, the face, the neck, the chest, the abdomen, the pelvis, or the limbs; and a large number of cases are reported in full. The proportion of wounds inflicted with criminal intent is very great—no less than 120, as compared with 96 the result of accident. There were in addition 6 suicides. This fact shows that, however much may have been

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[1] *Annali Clinici dello Ospedale dei Pellegrini di Napoli,* vol. ii, fasciculo 5 and 6. Decembre, 1872.

already done for the kingdom of Italy, there is still room for improvement in its educational and in its police arrangements.

Although, as we have said, so many cases are related *in extenso*, there are few which are of any special interest; and we notice little or nothing that is new as regards their treatment. If we mention the use of oil of mustard seed as a rubefacient, and the lettuce poultice as an anodyne, we have named the only trifles which have any novelty. We shall, however, give an abbreviated account of two or three cases which may possibly interest our readers.

The first is in the section devoted to wounds of the neck, and is worthy of notice as being an instance of successful ligature of the internal carotid.

Giulio M—, of Ravenna, aged 26, a music compositor, was admitted into the hospital at 7 p.m. on the 10th of July. Professor Favaloro, who was on duty, reported that there was an incised wound, passing from the left side of the neck backwards to the right side, deep enough to divide the flesh and the great arteries. It had been caused by the cut of a razor. Professor Barba treated the case in the first instance at the patient's own house. On the 11th of July a report was handed in to the effect that two days previously G. M. had had the left internal corotid and several smaller arteries tied, and the edges of the wound united by interrupted sutures. The operation had been performed in a masterly way. As the patient was a stranger, and had no one to wait upon him, he sought admission into the hospital. His head and neck were kept at rest, and the wound was treated with styptic lotions and ice. On the seventh day an abscess was detected near the insertion of the sterno-mastoid; a counter-opening was made and a drainage tube inserted. It had a good effect. The discharge escaped by the lower opening, the original wound became more healthy, and its edges coalesced. After six days the drainage tube was removed, when cicatrization was commencing in many points, the inflammatory swelling had almost disappeared, and the general condition of the patient was most satisfactory. On the 1st of August he left the hospital convalescent.

The next case is taken from the group of wounds of the chest. It is an example of recovery from a penetrating wound involving the lungs.

Mattia M—, aged 37, a machinist, entered the hospital on the 13th of August. The surgeon in charge, Olivieri Achille, gave the following report:—The patient had an incised and punctured wound produced by a knife on the left side of the chest, entering the lung. An attempt was made to close it by the first intention. The edges were united with a needle, bladders of ice were applied, and the patient placed upon a diet of asses' milk. On the fourth day respiration became painful under the left mammary region. Percussion

gave a dull sound from the angle of the left scapula to the base of the thorax. The vesicular murmur was weakened, and there were abundant mucous râles. The margins of the wound were much swollen for about six inches, the swelling gradually diminishing towards the iliac crest of the same side. Occasionally there was hard cough and evening fever. On the 20th two incisions were made, one in the internal and upper angle of the wound, the other about the middle of the lower margin. An artery of small calibre was tied, and the wound was treated with styptic lotion. The next day there was an abundant discharge of pus; and a counter-opening was made in the seventh intercostal space, giving exit to another collection of pus. This treatment was attended with success. On the 23rd incipient granulation was observed. Subsequently dry dressings were used, and granulation and cicatrization proceeded satisfactorily. On the 6th of September he was discharged from the hospital convalescent.

The following case of penetrating wounds in the abdomen may be taken as an average example of the numerous injuries from violence contained in this report; the reader will therefore perceive that the practice of the Pellegrini Hospital, recorded in this volume, embraces many surgical cases of more than ordinary interest. We have selected this one because the compiler of the report was himself the surgeon under whose care it fell.

"Casimir B—, of Capua, aged 47, a musician, was admitted into the hospital at 1 a.m. on the 25th of March: I was myself on duty, and reported on his case as follows:—He had three punctured and incised wounds; the first was in the left side, penetrating into the abdominal cavity, with protrusion of the peritoneum and probable lesion of the internal viscera. The second on the right side of the scrotum, reaching the abdominal cavity, so that the peritoneum became visible, and a livid spot was noticed on the right iliac fossa, caused by extravasation of blood. The third wound was upon the radial border of the lower fourth of the right forearm, deeply penetrating the muscles. These wounds were produced by blows from a foil. The patient fell, fainting; his antagonist attacked him again, giving him the blow which passed from the scrotum to the abdomen, and not satisfied with his second assault, struck him once more over the arm. I enlarged the wound in the side to prevent the strangulations of the peritoneum, and then plugged it with a compress spread with simple cerate. The second was treated in a similar manner. With the index finger of my right hand I tried to make the plug extend as far as possible from the scrotum into the abdominal cavity. The third wound, on the forearm, was closed by the first intention. Two bladders of ice were applied to the abdominal wounds, and small pieces of ice given internally. The patient was placed in the supine position with the thighs flexed upon the pelvis.

This treatment was maintained for three days, when the plugs were withdrawn. The wound in the side was beginning to granulate. That in the scrotum was complicated by an abscess in the right iliac fossa, which was opened, and a drainage tube introduced for the sake of injecting the wound. Poultices were applied. There was obstinate constipation for some days and subsequently a discharge of fæcal matter was noticed from the scrotal wound. This was followed by superficial erysipelas, and the formation of an abscess in the perinæum. The abscess was opened on the 12th of April, and a second drainage tube introduced. From these openings there was an abundant escape of pus, mixed with fæcal matter; but there was no inflammation of the bowels, which acted naturally. The dressings were changed four times a day. On the 15th the fistula had closed, and on the 17th the drainage tube was removed. During this time all the wounds were healing favourably, and the general condition of the patient was excellent. The drainage tube which was first introduced, from the original wound to the iliac fossa, remained until the 23rd of May, when it was taken out in order to allow the walls of the track to unite. On the 26th, as adhesion had not taken place, the fistulous track was opened up by incision, and then granulation and cicatrization went on, though slowly. Ultimately the patient made a complete recovery, and was discharged on the 15th of July."

The previous cases have been selected from the fasciculus on wounds; we shall give but one from the fasciculus on burns. The number of cases recorded is only twenty-one—of these four proved fatal, death being due rather to the extent of surface involved in the injury than to its severity.

Pascal L—, aged 16, a mason, was brought to the hospital on the 1st of June. Professor Sabelli reported as follows:—There were burns of the first and second degree, produced by quick lime, over the whole of the left hand, also upon the lower limbs, covering the outer part of thc left thigh, the front and sides of both legs and the feet. Morphia was given, the burns dressed with a liniment composed of chalk and oil, and a bath ordered. The same treatment was continued for a week. On the third day a febrile reaction set in, and on the 8th the characteristic appearances of chicken-pox manifested themselves. These lasted two or three days, and then subsided. From the 11th to the 20th there were tetanic symptoms, but without trismus. On the 22nd there was slight trismus, and the general condition of the patient was worse. Hypodermic injections of curara were used four times a day, and the opium continued. Under this treatment the trismus and tetanus were subdued. The superficial sores were in a great measure dried up, though in some places suppuration continued; and the patient's general condition was somewhat improved. On the 26th high

fever again set in. The left hand was put upon a splint to stretch the cicatricial tissue. On the 27th an abscess was discovered in the abdominal parietes, bed-sores formed on the sacrum and trochanters; there was profuse diarrhœa and prostration; and the patient gradually sank and died on the 4th of July.

In a foot-note on the occurrence of chicken-pox in this case we are told that small-pox, modified small-pox, and chicken-pox were prevalent in Naples at the time, and that the burn assumed the character of the prevailing epidemic. If this is intended merely to account for the intercurrent attack of chicken-pox, well and good; but if it is intended to convey the idea that there is some connection between these diseases and burns, so that they are more liable to manifest themselves in those who are suffering from burns, the doctrine is one which we cannot admit. There is no proof of any such susceptibility.

It will be observed that in the treatment of this case a bath was frequently ordered. Subsequently, in discussing the mode of dealing with such injuries, much stress is laid upon its use. But it is nowhere mentioned whether the bath should be hot or cold. Local bathing with cold water is expressly recommended, and we should infer from the context that the general bath was also to be cold; and yet we should have thought that the warm bath would have been the most suitable, and would have given the most relief.

The number of fractures reported for the quarter is very small—only eight; and there is none of sufficient importance to induce us to make quotations. Indeed, the whole section appears to be of rather a fragmentary character.

At the end of each section the compiler gives a *résumé* of the particular dangers, difficulties, and modes of treatment of each class of injuries. This will be found useful to the student, by drawing his attention to the important points in the several groups of cases. The consideration of the question as to whether wounds of the abdomen should be closed, or kept open by means of a plug, is continued in this volume, and, although the writer arrives at no positive conclusion, he inclines to the use of plugging in the more severe cases, while in the simpler he would aim at union by the first intention.

On the whole the report is a satisfactory one both as regards the proportion of cures, the freedom from hospital scourges, and the painstaking manner in which the cases have been reported.

Woodward on Cancerous Tumours.[1]—The origin of cancer is, as the author truly says, a question surrounded by the greatest obscurity; and this, far from being removed, is rather deepened by the

[1] *On the Structure of Cancerous Tumours, and the Mode in which Adjacent Parts are involved.* The Toner Lectures, 1873. By J. J. Woodward, Assistant-Surgeon U.S.A.

latest observations. We have at present before us three distinct views as to the origin of the cells of a cancer, each supported by authorities of equal eminence. First, we have that of Thiersch and Waldeyer, who believe the cells of a cancer to be the direct descendants of pre-existing epithelium; secondly, that of Köster, that the cells of cancer are derived from the endothelium of the lymphatics; and, thirdly, that of Classen and others, that all the new elements of cancerous growths are no other than migrated white blood corpuscles. Others, again, hold opinions composed of various combinations of the three primary ideas, with addition sometimes of Virchow's theory as to the origin of cancer cells from the proliferation of fixed connective tissue corpuscles. Using the word cancer in the limited sense in which it is now usually employed, all observers are agreed as to certain points in the microscopic anatomy of the growth. That the cells are of a more or less distinctly epithelial type, that they are enclosed in a fibrous stroma arranged so as to form large alveolar spaces communicating with each other, that the growing margin is characterised by a small-celled infiltration of the surrounding tissues, and that the whole growth has some intimate anatomical relation to the lymphatic system, are points on which all are agreed; but beyond this we can hardly hope for any uniformity of opinion at present. The author of this lecture has devoted much time and independent observation to these questions. He has nothing to add to our knowledge concerning the relation of the growth of the lymphatic system. It is chiefly to the relation of the small-celled in filtration to the fully-developed cells of the cancer that he has directed his attention; and here he agrees with Classen, Rindfleisch, and others, that the cancer cells are developed from wandering white blood-corpuscles, although a part of the growth may be formed by proliferation of the pre-existing epithelial cells of the part. One of the author's observations is undoubtedly of great interest. He states that "in almost every section of epithelial cancer in the museum collection I find among the epithelial cells numerous unmistakable wandering corpuscles, often fixed by the reagents used, with their processes extended as in the act of migration. If Biesiadecki is right in believing that the ordinary growth of epithelial cells is effected not by cell multiplication, but by these wandering corpuscles becoming fixed and developing into epithelial cells, we can readily understand the significance of this circumstance." The author seems, however, to have observed but little of any intermediate stages between the small round cell and the epithelial cell of cancer; and without this the evidence that they are so intimately related is somewhat imperfect.

We cannot quite agree with the author in including one case at least from which he exhibited specimens under the name of cancer. The tumour was from the heart of a young woman, and weighed

five pounds. It was made up of "nearly normal gland-ducts and gland vesicles pushed apart by a luxuriantly developed connective tissue stroma, through which numerous small cells were scattered." He compares this with a case in which he found a somewhat similar condition of the mammary tissue surrounding a scirrhous cancer of the breast. But the difference between the two is much greater than at first appears. In the first the whole change occurred uniformly throughout the mamma without any apparent cause. In the second the mamma was the seat of an unnatural activity resulting from the presence of a cancerous tumour, and the small-celled infiltration and dilatation of the acini of the glands are not unnatural results of such a cause. There is no evidence to prove that the simple growth preceded the malignant; in fact, it seems to us that the probabilities are vastly in favour of the opposite view. On the whole we cannot say that this lecture adds much to our knowledge of this difficult subject. The author has reviewed the work of others tolerably completely; and although he seems to have come to a definite opinion himself as to the mode of growth of cancer, he has little direct evidence of his own to give us upon which he has founded that opinion.

Waring on Bazaar Medicines and Common Medical Plants of India.[1]—The nature and scope of this little work are well indicated in the title, but a few observations are required to point out the objects which Dr. Waring seems to have had in view in presenting it to the general public. It was first issued fourteen years ago, in India, for the use of the District Vaccinators of Travancore, whose sphere of action was far removed from regular medical aid, and it was printed in the English and in the native language on opposite pages. It was subsequently reprinted in the native language alone by the London Missionary Society's press at one of their Missions for the use of the Catechists and others engaged in missionary work. Dr. Waring thinks, and we believe rightly, that the diffusion of some trustworthy information amongst the natives of India, in order to enable them to use with advantage the medicinal substances procurable at a cheap rate at the bazaars, or even from the road-sides or gardens in the neighbourhood of the stations, might lead to the avoidance of errors and to the amelioration of suffering. He further suggests that a combination of medical knowledge and the method of applying it, with the religious teaching of the missionaries, or in other words, making religion and medicine go hand in hand, would very much promote the success of missionary efforts. If

[1] *Remarks on the Uses of some of the Bazaar Medicines and Common Medical Plants of India, with a full Index of Diseases, indicating their treatment by these and other agents procurable throughout India: to which are added Directions for Treatment in Cases of Drowning, Snake Bites, &c.* By EDWARD JOHN WARING, M.D. Second edition, pp. 212. London, 1874.

addressed to English readers in reference to the medicinal agents of Great Britain, where medical science is widely known, and where medical labour is abundant, such a book as that of Dr. Waring would savour of empiricism, but, addressed as it is to an immense but scattered and ignorant population living in a magnificent and boundless region where the earth yields alike poisons and medicines in abundance, it is not only eminently useful, but it supplies a want which must be felt by all English residents or visitors who are in any way brought into contact with disease and suffering, either in their own families or among the natives who are living near them, in districts where they are far away from medical aid. Dr. Waring gracefully acknowledges that much of the information he possesses as to the properties and uses of Indian drugs has been derived from native sources, and he thinks, not without reason, that he is now repaying this obligation by furnishing in return a considerable amount of knowledge as to the use of some drugs of which the natives themselves had previously no idea.

Dr. Waring's book mainly consists of an alphabetical list of the bazaar medicines and Indian plants in general use, giving the botanic or other sources from which each is obtained, and the diseases or injuries for which each is adapted; the methods of administration or application are indicated, and the native names are given, accented, in the different dialects used in India. There is also an index or synopsis of diseases, containing brief directions as to the course to be adopted, in the absence of medical advice, in certain contingencies, together with directions for restoring the apparently dead from drowning, and a summary of treatment (reprinted from Dr. Fayrer's splendid work, 'The Thanatophidia of India') of persons bitten by venomous snakes.

Fothergill on the Maintenance of Health.[1]—This work is avowedly addressed to the general public, and therefore does not require any extended notice at our hands. Nevertheless, it is only fair to observe that while the public remain as ignorant as they hitherto have been of some of the most ordinary laws of individual health and of general hygiene, the perusal of works such as the present may be of considerable benefit, especially at a time when such matters are beginning to force themselves, as it were, on the attention of the community. As may be expected, Dr. Fothergill's pages teem with matters which are familiar to the medical profession, although it must be admitted with regret that even among ourselves many questions of interest in hygiene are still unsettled; and the writer leaves such subjects pretty much as he finds them. In reference to the alcoholic question, for instance, he propounds

[1] *The Maintenance of Health, a Medical Work for Lay Readers.* By J. Milner Fothergill, M.D. Pp. 399. London, 1874.

views which, as he himself foresees, will please neither the extreme advocates of abstinence on the one hand, nor the apologists of drinking habits on the other; although he seems to us to take a very rational course in showing the truth to lie between the two. He is probably right in recommending people who either dislike alcoholic drinks or find themselves the worse for their use, to abstain from them, while at the same time he claims an equal liberty for those who find them useful or agreeable in moderation, to persevere in their consumption. As to tobacco, of which Dr. Fothergill professes himself a consumer, he holds pretty much the same opinions, and while admitting, as every one does, that the weed is a poison, he does not condemn its moderate employment.

On looking over the book, we find that all the other matters of personal or social importance are discussed in the same fair and impartial spirit; the style is good, and the remarks are often quaint and amusing; the rules laid down are generally in accordance with sound physiological principles; and on the whole we think that lay readers may with advantage read the volume and ponder over the practical lessons it contains.

Murri on the Theory of Fever.[1]—This essay, which is altogether of a speculative character, is devoted to the consideration of the essential nature and causes of fever, and deals with questions which have from time immemorial engaged the attention of physiologists and physicians. Dr. Murri introduces the subject by an elaborate review of the principal opinions at present maintained by pathologists, most of whom regard fever as consisting essentially in elevation of temperature, due in the first instance to some affection of the nervous system. One great argument brought forward in support of this view is the elevation of temperature said to be caused by section of the spinal cord, but Dr. Murri questions the truth of the statement as to the constancy of this effect, or at least he shows that, under certain conditions, cooling and not heating follows this operation. He adduces a number of experiments made by himself on dogs, when the temperature, both of the rectum and of the external parts (as the fold of the thigh) were carefully noted, and by these experiments it appears that a lowering of the temperature of the periphery of the body followed the section of the cord. The researches of Dr. Murri, which seem to have been very carefully conducted, have led him to arrive at certain conclusions somewhat at variance with the opinions generally entertained, and he thinks that, on the whole, the *chemical* theory of fever (which

[1] *Sulla Teoria della Febbre. Indagini del Dott. Augusto Murri Aiuto alla Cattedra di Clinica Medica nella R. Università di Roma.* (*On the Theory of Fever.* Researches by D. Augusto Murri, Assistant Prof. of Clinical Medicine in the University of Rome.) Pp. 132. 1874.

implies the agency of some material, introduced from without, upon the animal fluids) is preferable to the *neuropathic*, or that which places the origin of fever in some derangement of the nervous system, especially the sympathetic. On this view, the elevated temperature, which undoubtedly characterises fever, is not the essence of the disease, but rather the effect of the chemical changes primarily induced in the blood; and therefore the cure of fevers is not to be sought merely in agencies which reduce the temperature, but in those medicines which counteract or destroy the organisms on which fever essentially depends.

Thompson on Free Phosphorus in Medicine.[1]—Phosphorus, in its chemical relations, holds a kind of middle rank between the metallic and the non-metallic bodies, and it is certainly closely allied to arsenic, which, by modern chemists, is ranked among the latter. Phosphorus and arsenic are alike combustible, although with very different degrees of intensity, and both burn spontaneously in chlorine gas; both form with oxygen two distinct acids, both combine with hydrogen to form a gas peculiar to each; both are tribasic or triatomic; both exhale a peculiar smell when burning, arsenic having the smell of garlic and phosphorus presenting somewhat of a similar odour. In physical characters, however, they are different, one being hard and brittle, the other, in its usual state, soft and plastic; and in their physiological relations the difference is still more strongly marked, for whereas arsenic cannot be said, under any circumstances, to form a normal constituent of the human body, phosphorus, on the other hand, is an essential element in the composition of all vertebrate animals, and it is no doubt owing to the latter circumstance that efforts have been made, from the earliest periods of its discovery, to introduce it as a medicine. The great danger attending its use, however, and the actual occurrence of fatal cases of poisoning by its agency, have deterred the profession from employing it, and it was not till very late years that its internal administration was revived, and still more recently that this powerful agent has acquired the position of an authorised medicine in Great Britain by its admission into the Pharmacopœia.

Mr. Thompson's book, which we may at once characterise as a very able monograph, is presented to the profession, as he tells us in his preface, not so much in the form of an original work as of a *résumé* of what is at present known of the use of phosphorus in medicine, illustrated by his own clinical experience. It is admitted that it is a poison, and a dangerous one, but Mr. Thompson's object

[1] *Free Phosphorus in Medicine, with special reference to its Use in Neuralgia; a Contribution to Materia Medica and Therapeutics.* By J. ASHBURTON THOMPSON. Pp. 275. London, 1874.

is to point out the means of avoiding the accidents which have sometimes attended its use, and at the same time to indicate the methods of employing it with advantage in the treatment of disease, and to render its operation determinate and uniform. Its pharmaceutical preparations are carefully described, because the efficacy, stability, uniformity of effect, and safety of the forms in which it is administered, depend, in Mr. Thompson's opinion, entirely upon the integrity of the active ingredient they contain.

Phosphorus, as is well known, was discovered towards the end of the seventeenth century, and was at first obtained from urine, and it was only in the year 1774 that it was found to exist in bone, from which it is now almost exclusively extracted. Its use in medicine dates a few years subsequently to its discovery, the original source of its production no doubt leading to its therapeutical employment, and from time to time it has been recommended by various medical writers, chiefly on the Continent. The effects of phosphorus upon the human system have been rather prominently brought into notice of late years in consequence of its employment in the manufacture of lucifer matches, and of the illnesses produced among the work-people who handle it or imbibe the fumes. The results recorded, however, differ widely in different cases, for while necrosis was developed in some, and miscarriages took place in pregnant women, other persons, on the contrary, seemed to enjoy remarkably good health and spirits. These and other circumstances have brought the toxicological as well as the therapeutical qualities of phosphorus again into notice, and several medical practitioners and writers, among whom are Dr. Cotton, Dr. Hughes Bennett, Dr. Broadbent, Dr. Eames, Dr. Thorowgood, and Mr. Thompson himself, have given us their experience of it.

The preparation of phosphorus for medical administration is, as has already been mentioned, a subject of very great importance, and the want of due precaution in the matter has probably been the cause of its frequent failure or of its deleterious operation. Owing to the peculiar physical characters of phosphorus it cannot be administered conveniently by itself, that is to say, without being dissolved in some menstruum adapted for its reception, and capable of subdividing it so as to insure its efficacy without changing its elementary nature. For, it must be remembered, *free* phosphorus must be given, and its conversion into hypophosphorous, phosphorous, or phosphoric acids, either increases its toxical effect or destroys its efficacy altogether.

The solvents which have been employed are various oils, some vegetable, some animal, and also ether, chloroform, naphtha, bisulphide of carbon, aërated water, acetic acid, and sulphur. Of these it is found that bisulphide of carbon and chloroform are not well borne by the patients, and sulphur and acetic acid are said to be entirely

unsuited medically to act as solvents. But another mode of employment is in chemical combination with zinc, as the phosphide of that metal, and this salt presents several advantages in practice. All substances which facilitate the oxidation of phosphorus are to be avoided, for, according to Mr. Thompson, phosphoric acid has none of the therapeutical properties of free phosphorus, and the lower degrees of oxidation are more intensely poisonous than phosphorus itself. Olive oil, in which phosphorus may be dissolved, has the property of absorbing a very large proportion of oxygen, and therefore when it is employed as a solvent a preparation is obtained containing a mixture of free phosphorus with hypophosphorous and phosphoric acids. This solvent is accordingly contra-indicated, and it is found, both on chemical grounds and upon those of convenience of administration, that the three best solvents are cod-liver oil, alcohol and ether, and the first is that which Mr. Thompson has most commonly employed. It appears that cod-liver oil preserves the phosphorus in a free and active state sufficiently long to allow of its efficient administration, and as this oil does not absorb oxygen in the same manner as the vegetable oils, it is much less dangerous. Conversely it is found that the cases where poisoning by phosphorus has occurred have been aggravated in danger when a solution in vegetable oil has been employed. For the reasons just mentioned Mr. Thompson objects to the use of the *Oleum Phosphoratum,* which has long had a place in the pharmacopœias of France and Prussia, and has very recently been admitted into the Appendix of the British Pharmacopœia. The formula he proposes, as being most efficient and least dangerous, is that in which phosphorus is dissolved in cod-liver oil, the proportions being one grain of the former to an ounce and a half of the latter, so that one drachm contains one twelfth of a grain of phosphorus, and Mr. Thompson considers this a full but useful dose. In the form of pills, phosphorus may be given in the reduced or pulverulent state, in solution, or as phosphide of zinc. The pills in the Appendix to the British Pharmacopœia are ordered to be prepared with phosphorus, balsam of Tolu, and yellow wax, but Mr. Thompson considers this and other forms of pill hitherto devised to be open to objections, as being occasionally dangerous in use, and he thinks it desirable that some method should be found of minutely dividing the phosphorus without exposing it to oxidation, and he has at length, he thinks, succeeded in procuring the preparation of pills with all the necessary precautions.

Among the chemical compounds of phosphorus which are calculated to produce the effects of phosphorus itself, Mr. Thompson thinks that the phosphide of zinc, prepared by passing the vapour of phosphorus over boiling zinc in an atmosphere of dry hydrogen, is the best. When introduced into the stomach it is

decomposed into phosphuretted hydrogen and a salt of zinc, and the former, being a dialysable gas, is capable of affording the same results as free phosphorus. The composition of zinc phosphide is definite, and is expressed by the formula Zn_3P, and the atomic weights of those two elements being nearly equal, it follows that the formula expressing their combination shows the phosphide to contain one fourth part of its weight of phosphorus. Thus, assuming the atomic weight of zinc to be 62, and that of phosphorus also to be 62, then $186\ (62 \times 3) + 62 = 248$, and $248 \div 4 = 62$.

With regard to the effects of phosphorus as a medicine, it would appear from recent investigations that it acts as a stimulant, and that it is especially serviceable in the treatment of neuralgia, in which disease Mr. Thompson has frequently employed it. He gives a tabular view of fifty cases of neuralgia, arising from various causes, in which phosphorus was employed, and the general results seem to have been very favorable. In locomotor ataxy phosphorus has been employed without success, and in sclerosis of the posterior columns of the spinal cord (on which the disease in question is supposed to depend), it is difficult to understand how it can act beneficially. In general brain and nerve-exhaustion, in phthisis, and in some cutaneous diseases, it has also been used with considerable success, and it is now being employed by various physicians in those affections. In impotence its effects are doubtful, for although it certainly acts in some circumstances as an aphrodisiac, its power in this respect seems to be developed only when the dose is a poisonous one. In cholera, amaurosis, cataract, and glaucoma, in softening of the brain, and epilepsy, in all which it has been employed, we should very much question its efficacy, and it is almost needless to remark that it is altogether contra-indicated in inflammatory or hypersthenic diseases.

With these observations we must at present leave the subject, which is now, however, undergoing active investigation; but we must repeat that, according to the experience hitherto obtained, the dose of phosphorus is one twelfth of a grain, the best form of administration is that of pill or in cod-liver oil, and it should not be given on an empty stomach.

Thorowgood's Materia Medica.[1]—The writer of this small treatise puts it forth as a supplement to the 'Pharmacopœia' for the use of students preparing for their examination in materia medica, and he assumes that the 'Pharmacopœia' is a favorite volume with such gentlemen, and, consequently, he very briefly describes the characters and modes of preparation of officinal or, as he curiously calls them, "official" medicaments. The assumption made we, however,

[1] *The Student's Guide to Materia Medica, in accordance with the latest Issue of the 'British Pharmacopœia.'* By J. C. THOROWGOOD, M.D. London, 1874.

apprehend is more or less fallacious; the 'Pharmacopœia' is neither very readable nor very attractive to students, and, as a rule, these seekers after knowledge look to their books on materia medica to teach them whatsoever is contained in the official volume that is required to satisfy their examiners. In the hands of such individuals Dr. Thorowgood's 'Guide to Materia Medica' would be insufficient, and, in general, we can only regard it as a mere outline of its subject, sufficient, may be, as a syllabus to students diligently attending lectures and taking notes, but inadequate to the want of those who would at all trust it in preparing for an examination. For besides the scant description of the physical features and qualities of drugs, of the processes of manufacture and of botanical relations and characters, the account of the chemical composition and of the chemistry of pharmaceutical processes is too brief to serve the purpose it professes to have in view; and with respect to the notes of the author on the uses of the various drugs we see in them none of the "general principles of therapeutics" which he tells us in the preface "have been mainly regarded." Of the two divisions of the treatise that devoted to inorganic materia medica appears to us the more satisfactory.

We do not desire to condemn the book, for, so far as the information actually contained in it is concerned, it is a good outline; but in our opinion, considering the present ample supply of treatises on materia medica, it does not make out, to use a somewhat hackneyed modern phrase, its "raison d'être."

Original Communications.

I.—On the Etiology of Leprosy. By G. Armauer Hansen, Assistant Physician to the Leper Hospitals at Bergen, Norway.

The researches on which I shall here report are made with special respect to the occurrence of leprosy in Norway and to the opinions of Norwegian inquirers. As to these opinions being naturally just the same as those maintained elsewhere, this will hardly interfere with the general bearing of my arguments. These are discussed at full length in my report to the Medical Society at Kristiania; I cannot here enter so largely on details, and so my assertions may sometimes seem to be too little supported, but what I consider as essential facts will be brought forth with sufficient details for the reader to form his own judgment.

Leprosy is considered by different Norwegian writers as—

1. Not specific and hereditary (Danielssen, Boeck, Hoegh, Conradi, Bidenkap, &c.).
2. Not specific and not hereditary (Hjort).
3. Specific, miasmatic, and not hereditary (Hohnsen).
4. Specific, contagious, and hereditary (Lochmann).

What appears most striking in this discrepancy of opinions is that heredity is admitted as most essential as well by advocates for the non-specific nature of leprosy as by advocates for its specificity; and on the other hand, that one who considers it specific and another who considers it non-specific, do both deny its heredity. None of the writers on the subject, and this applies also to foreign ones) make any distinction between transmission to offspring of a specific and of a non-specific disease. And yet there is, in this respect, an essential difference in the phenomena, if we take as specific those diseases which are usually considered to depend on the operation of a distinct poison on the organism, whether this poison be a chemical one or a low form of organic life, including thus all parasitic diseases under this category. Of these specific diseases we distinguish acute and chronic ones, contagious and not contagious; all may be named infectious.

In direct contrast to the infectious diseases there is a long series of abnormal states which arise independently of any special influence, but which rather depend on a production of the organism not,

or only occasionally, influenced by external circumstances, such as many abnormities of skin, eyes, &c., many neuroses and mental diseases. It is satisfactorily demonstrated that all these states may be, and very commonly are, inherited. The phenomena connected with the hereditary transmission of these states are completely analogous to the phenomena by inheritance of physiological qualities.[1] In no case we consider the inheritance as depending on the transmission of any specific matter producing the same consequences in offspring as in parents, but as transmission of structural peculiarities. We use even the heredity of normal and abnormal mental peculiarities as a very weighty argument against the belief in anything specific-psychical, whereby every one at his birth, or perhaps before, becomes, so to say, infected.

If we now turn to the specific diseases, and examine how the case stands in respect of their transmission to the offspring, we need say little with respect to the unquestionably parasitic disorders, such as scabies, favus, &c., or of the acute infectious diseases. Although in many cases it would be a tolerably easy task to construct family tables exhibiting individuals in two or more generations who have suffered from scabies, ague, measles, &c., still no one has ever supposed heredity to exist in these diseases; they are in each case considered to be produced by the influence of the specific morbific matter. Variola can be transmitted from the mother to the unborn child, and so can, perhaps, some of the other acute infectious diseases, but this is regarded as contagion, no one thinks it to be inheritance.

Syphilis may be considered as a type of specific and contagious chronic diseases, and syphilis is generally considered the most exquisite example of an hereditary disease. And though the phenomena attending its transmission to offspring are, at least in most cases, just the same as those connected with variola, so the specific contagious disease is transmitted to the ovum. When it is sometimes assumed that congenital syphilis occasionally does not appear until some years after birth, nay, even till early youth, it is not, as far as I have been able to gather from literature, made apparent that the individuals in question have not had visible symptoms of the disease at their birth or shortly after it, and still less that they have had no syphilitic affections of internal organs. And to prove this must be difficult, if not impossible. It is a well-known fact that in autopsy often reveals to us a far-developed syphilis in internal organs, while no symptoms have as yet appeared outwardly in children of syphilitic parents. It would carry us too far out of the way, to enter upon

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[1] Lucas, 'L'Hérédité naturelle.' Sedgwick, "Sexual Limitation in Heredity," 'Med.-Chir. Review,' 27, 28, 31, 32. Darwin, 'The Variation of Animals and Plants under Domestication.'

further details. I shall therefore only place side by side the chief phenomena attending the transmission to offspring of *Hereditary states* and of *Syphilis.*

1. The transmission is very often atavistic.	1. Atavism is not known.
2. Hereditary states manifest themselves at birth or in after years, and in the latter case mostly in corresponding ages in parents and descendants.	2. Congenital syphilis appears outwardly, when the child has not already died in utero, at or shortly after birth. At all events it does not appear to be established beyond doubt that no affections, internal or external, appear until in later life. There is no question of corresponding ages.
3. Hereditary states are often completely, or to a great extent, limited to the same sex.	3. There is no sexual limitation.
4. There is correspondence between the parts affected in parents and children.	4. There is no such correspondence.

These striking diversities must make us consider whether there be a real difference between the transmission to offspring of the acute specific contagious disease variola, and that of the chronic specific contagious disease syphilis. Upon a closer examination of known facts and upon grounds theoretical it will be found, I believe, that the incongruities are easily accounted for if the transmission is by inheritance in the one instance and by contagion in the other, and so in the latter in conformity to the transmission of variola.

One has only for a moment to take it for granted that syphilis is generated by a chemical poison, or rather by a parasitic organism, to admit the difficulty of forming a clear idea of a parasite being inherited.

Ergotism and pellagra are chronic specific non-contagious diseases. Pellagra is thought to be hereditary, not so ergotism. As to the etiology and the alleged heredity of pellagra, I beg leave to refer to Hirsch, 'Handbuch der historisch-geographischen Pathologie,' i, p. 478, &c., from which it must appear evident that there does not really exist any heredity. Further, pellagra is not transmitted to the ovum by contagion, a circumstance quite in harmony with the fact of its not being contagious at all.

The transmission to offspring of the diseases mentioned seems to me to stand in such decided relation to their etiology, as indicates a regularity which will most probably have universal application. My views on this head may be set forth as follows :

1. Those diseases which depend on a structural defect are hereditary.

2. Those diseases which are produced by a specific virus are of two classes, according to their character of being or not being contagious :

a. If the disease is contagious, it *may be* transmitted by contagion to the ovum, but is not hereditary.

b. If the disease is not contagious, it is not in any way transmitted to the descendants.

Though I cannot here go into particulars for evidence to support the above propositions, the reader will, perhaps, not find my conclusions too premature, when I conclude it highly probable that heredity and contagion are in direct contrast to each other. Engaged in investigations on the etiology of a disease the real cause of which is so entirely unknown as that of leprosy, the more circumstances I can bring forward to demonstrate the probability of one of the two alternatives existing or not, the more the probability of the other existing too will lose or gain. And this applies alike to other diseases. It ought to be remembered that Virchow always has maintained, specially for cancer and tuberculosis, that nothing specific is inherited but a disposition of certain tissues for the special disease. A predisposition to a special infectious disease can hardly exist; one man may catch syphilis, favus, &c., more readily than another, but that no one is born with a disposition for catching only one special infectious disease and no other, can hardly be disputed. On the other hand, contagion may be regarded as an unfailing test of a disease being produced by a special virus.

All attempts to point out the agents that in a specific or non-specific[1] way should directly generate leprosy, have hitherto failed. The discussion on the various alleged causes may, in my opinion, rightly be postponed till one of the two alternatives, heredity or contagion, be proved. I will mention, however, that all of them, including miasma and the eating of tainted fish, may, with a degree of certainty be shown to be insufficient to account for the occurrence of leprosy in Norway—may even be excluded. On the other hand, it can be shown that some of them, viz. uncleanliness, the occupation of the people, in short the whole mode of life of our peasantry, are very favorable to the spread of contagious diseases. For this we have well-established facts, such as the almost awfully common occurrence of scabies, though much on the decrease of late years; the spread of syphilis within families, in some instances most astonishing; the carrying about of typhoid fever by individuals among the fisher population congregated at certain times and places during the great fisheries.

It may also be mentioned that in the cases of leprosy in England and Germany represented as having been engendered in those countries, and of which I have read accounts and descriptions, the symptoms have not agreed with the symptoms of leprosy as they manifest themselves in Norway.

[1] In Norway the assumed non-specific origin of leprosy is called spontaneous.

The assumption of heredity is based on the relatively frequent occurrence of several lepers in the same families. Among the 528 lepers registered by Bidenkap[1] there were 135 without leprous relations in the ascending line or in collateral lines, 268 had leprous relations in collateral lines, 125 in the line of direct ascent. This last proportion is nearly the same as that calculated by Dr. Hoegh for the whole country. Bidenkap says, "It seems thus to be something constant, applying to the whole country or to any single part of it, that from one fifth to one fourth part have had leprous ancestors, and at least about one fifth leprous parents." This seems to be confirmed by the proportion among the 210 cases now registered by me, among which 51 have leprous relations in the line of direct ascent, 50 no leprous relations, and 109 have them in the collateral line. My tables are taken from another part of the country than Bidenkap's, which seems still more to confirm the rule. The proportion, however, falls out very differently in several smaller districts, as it varies from one half to one twentieth, while in some places there are none with leprous ancestors; and it is, perhaps, remarkable that where the appearance of leprosy may be considered as relatively of recent date, there is very seldom any relationship between the lepers, nor do the latter descend from leprous families of any other places. It is not until leprosy has become endemic that the cases occur which might indicate heredity. If, now, the disease was contagious, this might have a very natural explanation, as the first case of leprosy acquired by contagion elsewhere would afterwards most frequently infect those in nearest intercourse, members of the same family or relatives. In any case the number of lepers for whom even with the strictest search no leprous ancestors can be discovered, and of those for whom no leprous relations can be pointed out, is so great that one fourth or one fifth of such as have leprous ancestors is too small a proportion to prove heredity. After what I have already stated as to the improbability of degeneration or special disposition to sickness in the people, or of any circumstance peculiar to us for contracting leprosy, it will by no means be unjustifiable to refer all cases without demonstrable leprosy in the line of direct ascent to those which cannot be classed in connection with heredity, and then the number of these cases will be so great that heredity will appear improbable.

With regard to pellagra, it has been endeavoured to prove heredity in the same manner by recording a great many family cases. But now it has been shown in respect of this disease that if individuals go beyond the influence of the home infection they do not only not get pellagra, but that even if they have previously suffered from it, the disease disappears under the altered conditions, while it

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[1] 'Norsk Magazin for Lágevidenskab' (Norwegian Magazine for Medical Science), 2nd series, vol. xiv.

breaks out again if the party concerned be again exposed to the action of the specific infection. According to the numerous cases which, supposing heredity in leprosy, must be attributed in this disease to atavistic inheritance, the hereditary disposition must probably also be very slow to disappear; the same must be the case if we suppose a degeneration accumulated through many generations, or any other abnormity which might manifest itself without previous disease in the family. From whatever side we examine the supposed heredity in leprosy, it is therefore not to be expected that it should cease to operate, as it were, all at once, if the leprous family should be transported to another soil.

Prof. Boeck[1] has, as is well known, while acknowledging this, instituted his investigations among our countrymen in the United States, and is of opinion that the result of these investigations must establish the heredity of leprosy beyond doubt. The United States must be considered as certainly the best locality or such researches. It is sure that there has been no leprosy previously in those places where our countrymen have settled, and we cannot adduce local circumstances nor the conditions of life as causes of leprosy. What was to be sought for in America in order to prove the heredity, was cases that might be referred to atavistic inheritance from immigrants who themselves were free from the disease. If no such cases can be found then the heredity will not be made more probable than it was before, and in considering the possible cases of this kind it would be necessary also in America carefully to exclude contagion, for there are not so few lepers with the disease in mild forms who have emigrated to America.

The condition here suggested is not fulfilled in the cases reported by Prof. Boeck from America; in none of these can we exclude the possibility of the disease having been brought from home or acquired by intercourse with lepers in America (Observation 4). I must here again refer to the cases of leprosy mentioned by Danielssen and Boeck, and again quoted by Boeck in his last article, in which the disease broke out in a Frenchman and in a Dutchman respectively six and ten years after their arrival from leprous districts. This establishes undoubtedly that the disease may not come to light until many years after the time when it was contracted. All Boeck's patients were born in Norway, and the longest time after the arrival in America until the distinct manifestation of the leprosy was fourteen years. But in the face of the two cases quoted in which no heredity can be maintained, I cannot admit that fourteen years is a sufficiently long interval to exclude the possibility of the disease being brought from home. If one has only to depend on the patient's own statement as to the date of origin of the disease, then it is according to my

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[1] 'Nord. Med. Archives,' iii, 1.

experience impossible to determine how great an interval may be allowed in this respect. We stand here before a great hiatus in our knowledge, which Prof. Boeck also admits, while he does not attribute so great importance to it as I do in opposition to the cases collected in America. But it is my conviction that also a great many of our lepers here at home are credited with too short a duration of disease. The patient's explanation is nearly always uncertain and hesitating. It happens rather often that anæsthetics indicate a very short duration of disease, as they reckon the commencement of the disease from the marked loss of sensation and the manifestation of atrophy. They have nothing to say about spots, and yet traces of the latter may often be observed with great ease; the patients have no idea that they have had large and permanent spots for a long time, for only such spots leave evident traces. Still more easily may this occur with slight transient eruptions, which, as a rule, have their seat on the back and on the extremities, especially with people who seldom or never wash themselves.

With tubercular patients it happens that they are conscious to having had slight eruptions or a slight discoloration in the face many years before the manifestation of the disease.

According to experience hitherto such slight but yet suspicious skin eruptions have most nearly the character of an erythema nodosum. I shall briefly quote some cases of the disease in order to illustrate this.

L. H—, No. 804. Mons. Randal, spotty leprosy. Had, about three years before the affection existing when he was admitted, an eruption of red spots, tender to the touch and disappearing after about three weeks; at the same time pains in his limbs. A similar eruption in the following year. Again in the following year in November a new eruption, and of the spots then produced there were at his admission January 1st, 1873, only slight traces, from which leprosy could not with certainty be diagnosticated. More certain signs of the disease were the swellings of the inguinal and axillary lymphatic glands, and a slight insensibility along the outside of the backs of both feet, of which the patient himself had no conception.

L. H—, No. 731. Aslak Ljom. Tuberculous leprosy. Admitted September 14th, 1869; dates his disease from the spring, 1868, when there appeared tubercula on his thighs.

In the spring of 1863 he had an eruption of red tender spots on the extremities, which lasted from four to five days; a similar but not so strong eruption in the autumn of the same year and in the spring of 1864.

Now, I consider that the leprosy in this last patient began in 1863, but he himself considers that it began five years later.

I have already, in 'Nord. Med. Archives,' communicated cases as proofs that many years after the smooth form may seem to

have come to a conclusion and the spots and likewise the insignificant anæsthesia have disappeared, or possibly the latter has remained stationary, a relapse may occur in the form of a tubercular eruption, and this may take place after the lapse of five, six, or perhaps more years. If, now, a patient has no knowledge of his spots nor of his anæsthesia, as was the case of the patient first mentioned, and as is very commonly the case even with much greater degrees of anæsthesia, or if he has had for a year or two slight skin eruptions which he has not much noticed, and as the relapse or the continuation may not take place until many years afterwards, I cannot consider ten to fourteen years as any security for the disease not having been taken from home to America. I do not, therefore, base the justification of my assumption in this respect on an unlimited period of incubation; on the contrary, there is reason, according to my other observations, for believing that this period is not so very long.

I suppose, therefore, that the Frenchman and the Dutchman, as well as Prof. Boeck's patients from America, have had symptoms of leprosy before the time indicated by them, without being aware of it. It must be remembered that it does not so seldom happen that patients with syphilis are in the same case. In the question of contagion this circumstance is also of great importance. Even if the apparent time of the duration of the disease in one case and its occurrence in another is far apart, the latter may still be attributed to infection from the former. Moreover, the cases of the Frenchman and the Dutchman are in my opinion sufficient of themselves to make Prof. Boeck's cases not conclusive for heredity.

We are here in this country not without localities that in a certain degree may be compared with America—I mean our towns in the west country—and we shall now see how the case stands in Bergen, where there are many who have moved in from leprous districts. The numbers are not so very small. Among the patients (about 250) at present in the Asylum No. 1 there are no fewer than 119 who have near relations dwelling in Bergen; namely, 318 brothers, sisters, uncles, aunts, and cousins. If the children of these are reckoned, the number may be safely taken at 500. It would next be important to know how many of the other inhabitants of Bergen, where the working population is mostly recruited from leprous districts, may be reckoned as belonging to leprous families, or only how great a part of the population comes from leprous districts. Neither of these points can be settled. But it is a certain fact that the immigration to Bergen from leprous districts is of old date, although it has much increased in later times, yet the number of immigrants must previously have been very great; we constantly find among the working classes people whose parents or grandparents were immigrants, and the very general custom of giving to children the father's first name, with son or daughter affixed as surname, bears testimony to this also. And when we

have now seen how many there are who have relations at present in the asylum, it is scarcely too much if we reckon those inhabitants of Bergen who belong to leprous races by thousands. Here are, therefore, rich materials for heredity.

As regards Bergen, no change of climate, and hardly any change in the conditions of life in other respects, can be adduced, as the large majority of immigrants belong to the working classes. A great number of these are quite as much exposed to injurious climatic influences as the population of the country districts, and sometimes even more. The dwellings are, to a great deal, quite as bad as in the country. The only thing which can be alleged as an advantage of removal is a relatively larger consumption of fresh meat and greater cleanliness. The gain is, especially in this last respect, considerable. The itch is, according to my experience of two years as a physician among 2000 to 3000 of the working classes, of very rare occurrence, while this disease is in the country districts still very frequent, although much less so during the last few years. According to the same experience the change of food seems, on the other hand, to have less influence, for chronic derangement of the digestive functions is so common a disorder among the working population that it can scarcely be more frequent among the country population. As to the general state of health, it is incomparably worse in Bergen than in the country districts. Scrofulous affections are very common, and mortality is much greater. The soil seems thus to be quite as favorable for the fostering of leprosy as in the country districts, unless cleanliness is the chief factor.

But leprosy occurs in Bergen; and, then, the question presents itself whether we can say that it arises spontaneously there. If that is the case, there would be so much the greater probability for heredity being also an attribute. There have been, since 1856, noted more than fifty cases of leprosy in Bergen; the preponderating number of these (about forty) are people who either have gone out from the asylums and have settled in the town, or such as have had the disease in a manifest form only a few years after their immigration. These cases have no significance for us. As to the others, on which I defer communicating details, some of them ought to be considered as undoubtedly spontaneous cases according to common view, and for not a single case of leprosy among natives of Bergen can heredity be pleaded with any certainty; and this is quite striking when the materials for inheritance are so abundant, and when the spontaneous unspecific occurrence would appear to take place; these two modes of origin ought to go hand in hand. And yet the family conditions are what our attention has been almost exclusively directed to. This does not serve to support the theory of heredity.

But neither spontaneous occurrence nor heredity have much to

rest on when we can also show other cases for which contagion furnishes the most probable explanation. All information is wanting as regards possible contact with other lepers; and for this there has been in Bergen, and still is, tolerably abundant opportunity, as the inmates of the various asylums have more or less limited intercourse with the town, while the inhabitants of the town have also access to the asylums.

In the last few years the following four cases of leprosy have been quoted in Bergen—

1. Anders Hegrenaas, 63 years old, labourer, tubercular form; born in Jólster; states the duration of the disease to have been about three years in 1871. Does not know any leper in the family, which is also probable, as leprosy is very rare in Jólster, and he is not related to any of the lepers who have been registered since 1856. He has been in Bergen more than twenty years; quitted thirteen years ago his service at Lungegaard Hospital, where he had been bathing attendant four years, and therefore had been in close contact with the patients.

2. Anne Larsdatter Starefos, 51 years old, tubercular form; came twenty years ago to Bergen, and dwelt with a sister at Starefos, a farm which lies high up on the Floifjeld, close to the Asylum No. 1 and the Lungegaard Hospital. She was washer-woman at the Asylum No. 1 for ten years, and three years after she ceased to be so her leprosy was undoubted, but she had, however, long been suspected of not being healthy. She earned her living at that time by washing bottles in a beer brewery.

She was born on the farm Munie, in a mountain valley about two miles from Bergen. All people there are agreed that there never has been any leprosy in the valley. It is in any case certain that her parents, their brothers and sisters and parents, have all been healthy, and that she is the only leper known in the families from which her parents descend.

3. Joachim Berentsen, sailor, 26 years, tubercular form. His parents, who are still living, were born in Bergen. The parents of his sixty-years-old father (Lars Andersen) were both born in Indre Holmedal; they moved so early to Bergen that all their nine children, of whom the patient's father is the youngest, were born in Bergen. There are dwelling in Bergen grandchildren and great-grandchildren of them, who are all healthy. The mother's parents were from Horningdal, where there is no leprosy, and from Bergen. They had two daughters, of whom one has a healthy daughter, and the other has, besides our patient, two healthy children living. The patient resided at the age of fifteen years, in 1858-59, for half a year at Spidsöen, and during that time had intercourse with the peasants round about, and also with families where leprosy had existed. Kari Spidsöen, a girl of about the same age as the patient,

had then many leprous spots, and she and the patient played constantly together. It is not supposed that any more intimate connection took place. After his return to Bergen the patient worked on the Tyskebrygge, and very soon after his return, about half a year, his father remarked in his son an altered complexion, which aroused his apprehension of leprosy. This apprehension became certainty when his son went to sea in 1863. The patient himself became apprehensive of leprosy at the end of 1864, after having suffered from ague in Sulina, but he did not discover any undoubted marks of the disease until in the course of the year 1865.

4. Lyder Eriksen, carman, 27 years old, tubercular form. He was born in Bergen, a natural son of healthy parents born in Bergen; has a healthy sister born of same parents. The mother's father was from Aunland, in Sogri, and the mother from Hallingdal; the latter was not aware of there being any leper in her own family. The father's mother was of German origin, but his father was from Voss; he was healthy, but I have not been able to obtain any other information as to his family. There are dwelling at present in Bergen not a few relations of these families, and they are all healthy excepting our patient.

The patient was during a part of his childhood taken care of at the farm Lone, about two miles from Bergen. There were no lepers here, but at the neighbouring farm Espeland there was one, and the patient was often in contact with him. The patient was badly taken care of at Lone, and suffered *inter alia* from favus and scab. About eleven years ago he came back from Lone, but was there again for a short time the year after. The patient states that the disease broke out in 1870, that is, about four years ago; but his old grandmother, about eight years ago, called his mother's attention "to a bluish discoloration over the eyes, like a shade," which she did not like, and was of opinion that the lad was not all right.

As, now, heredity in leprosy is said to manifest itself until the fourth generation, and perhaps longer, nay even that it can operate without leprous ancestors, it is, of course, impossible absolutely to exclude this hypothetical heredity in any of the cases here cited, as it is almost impossible to exclude it in any case whatever in a country where leprosy is endemic. For the three last-enumerated patients it is, however, certain that there are two previous healthy generations in the family, and extremely probable at least that the respective patients are the only lepers in the families. None of them can be said to have lived in specially unfavorable conditions of life, except, perhaps, No. 4. But I have succeeded for them all in showing contact with lepers; and as regards one of them (No. 3), that the first indistinct traces of leprosy manifested themselves a very short time after he had been in contact with a leper.

It must certainly be admitted that the most probable explanation of the origin of these cases of leprosy must be sought in infection at the respective places where the parties concerned have been in contact with lepers. There is no holding point for miasma nor for infection by food, especially in Cases 1 and 3, and there remains for us only that mode of infection which we usually designate as contagion. For No. 2 (the washerwoman) there is good reason to suppose that the contagion may be communicated by things which have been in intimate contact with lepers.

If we now consider the occurrence of leprosy in Bergen on the whole, most of the cases are such as may be considered to be imported from the country; then relatively few cases affecting the natives of the town, which cases many would call spontaneous; no case which with any certainty could be referred to heredity; and, finally, four cases, two of patients born in the country district and two natives of the town, which with the greatest probability may be attributed to contagion. It must be remembered that, assuming the heredity of leprosy, we might expect with such abundant materials to find a large number of lepers. It appears to me that what is here pointed out does not tell for heredity, but even tells very strongly against it. The cases which stand without any reasonable explanation I have for my part no hesitation in referring to contagion, and this seems according to the data before us much more justifiable than attributing them to inheritance, for which we have not been able to find a single point of support on the spot.

We shall now transport our investigation to some of the homes of leprosy in the country districts. The task will be still the same—to weigh probabilities against each other; it is still here more difficult to obtain any certain proofs, and I suppose the reader will now agree with me that it is far from sufficient to show relationship between lepers in order to establish by inference the hereditary origin of the disease.

As above mentioned our peasants' mode of life in general is very favorable for the transmission of contagious diseases, and while leprosy is so frequent it is just as impossible to exclude contact with lepers as to exclude heredity in the third or fourth generation. The difficulty arises chiefly from the circumstance that leprous patients may suffer from the disease for years without themselves or others knowing it. In order to illustrate what long intervals in accordance herewith must be allowed for considering a contagion possible, and which makes it so difficult to discover any special occasion of contagion where opportunity for contact with lepers is so abundant, I shall begin with a case from Volden, in Sondmore.

As the only lepers in a farm Solliden we find Peder Solliden, his wife, and his wife's second husband; the second wife of the latter is still living and is healthy, as also all the children of these marriages.

On examining the surviving wife, as well as other people who had known the parties concerned well, I could not find that in three generations there had been any other lepers. The first leper, Per Solliden, rowed during the fishery for many years together with Elling Staaten and Martin Roünestad, who were both lepers, and there was unanimous testimony to Martin having been a leper for many years when he and Per Solliden were constantly chumming together both winter and summer, while Elling Staaten probably became leprous at a later date. Per Solliden died a leper in 1841; his wife married again; when she became a leper is not known, but she died a leper in 1856, and it was not until 1860 that her second husband was aware of his own leprosy.

Solliden is a mountain farm, as the name denotes, situated on the sunny side, and the situation must thus be considered as favorable. As no leprous relations could be indicated for any of the three, it becomes probable that they have been infected by contagion. In this case there would be a series of contagions for three cases, from the close of the year 1830 to 1860, which at first glance seems improbable. But I suppose that the woman who died a leper in 1856 was already a leper when her husband died fifteen years previously, which is no improbability, whether she had the disease in the smooth or tubercular form, and that she was not aware of it when she married again. Between her death and her second husband's manifest leprosy there is only an interval of four years. And as analogous to this I can cite the following case:—Knud Villa, Tresfjorden, in whose family no leper can be found, rowed during the fishery with Ole Sætre and lay with him in the same bed when employed in building work in the summer of 1858. In 1859 the leprosy of Ole Sætre was so far advanced that he was sent to Reknæs Hospital, and in 1864 Knud was no longer in doubt as to his own leprosy; he is a strong man and lives in tolerably good circumstances. Also in this case contagion seems to be the most probable explanation.

While in the cases mentioned no leprous relations could be indicated, we have in the following three leprous children of the same leprous father, where also contagion may be made probable, although the presumption seems to be in favour of heredity. All these have been in contact with a leprous servant girl from Stryen, who served at the farms Rake, Aflem, Bruvold, and Algældet in Indre Nordfjord. At Algældet she lay in the same bed with the daughter of the house, Malene, and the latter became diseased at home in 1850 or thereabouts; after her the father became diseased in 1859, and at last two sons in 1866 and 1867; both sons have lain in bed with their father. The idea of attributing this to contagion from the servant-girl may be combated by the fact that the girl when serving at Bruvold, while she was undoubtedly leprous, attended to the youngest son in the house, who is still living and is healthy.

But the notion may be corroborated by the circumstance that one case at Rake, Anne, the only one in the family, can be brought into connection with her, as Anne during the summer, while at the mountain pasture, lay in the same hut with the servant-girl when the latter was at Rake, and in the same hut people lie also in the same bed. Anne became aware of her disease about eight years after she had lain at the mountain pasture with the servant-girl.

While I remark that, according to observations of near relations for two of the cases reported from Bergen, it must be considered certain that the disease exhibited slight symptoms already four or five years before it was noticed by the patient, we shall not henceforth require to dwell further on the very important objection which may be drawn from the apparently long intervals occurring between the particular cases which may be supposed to have communicated the infection to each other. I have stated what I could discover to refute the notion, and I will in the following lines only insist on those other points which may have some influence on the question between heredity and contagion.

I have met with two cases of immigrants into leprous districts from non-leprous districts who have got the disease, and who both can be proved to have been in contact with lepers. One of them is Oline; she was born in Sunelven, where leprosy does not occur, so that inheritance may with tolerable certainty be excluded. Since her residence in Sombrefjord she has had frequent intercourse with many lepers, especially with those at the neighbouring farm, Langsten. The other is Petter Jensen, from the parish of Straud, where also leprosy does not exist. He has constantly attended to, and taken care of his leprous neighbour and brother-in-law, Petter Riksheim; he tended him when dying and as a corpse, and became leprous shortly after.

For these two cases it seems, therefore, that contagion furnishes the most probable explanation; they had both immigrated to leprous regions, and we come thereby to that view of the case which Holmsen defends, namely, that leprosy is not attached to the families, but to the place. I shall in the following lines adduce several cases which tell for that notion; but as I must repudiate an explanation by help of miasma or of special conditions of life in these places, I seek the probable explanation in contagion.

The above-named Petter Riksheim belongs to a family[1] in which there are many lepers. The family comes from the estate Hjelle in Orskoug; and in this part of Orskoug no leprosy occurs, as also the old Lars Ellingsen Hjelle maintains that leprosy never has been known at Hjelle nor in his family. All the members of the rather

[1] I use the designation race or family to distinguish in some degree between the greater groups of relations. By family I usually mean the collection of families descending from one pair; by race many families grouped together.

extensive family who have become leprous, have now removed westward to regions where the disease is at home, while those who have remained near the family home are free from the disease.

Still more evidently does this appear in a large family at Volden. It is a family that comes from the before-mentioned farm Solliden; the family is so large that not nearly all its members have been noted; theleprous Rasmus was of opinion that there were about 150 cousins, and of these there are again many children. Now, in all this extensively ramified family, there is only leprosy in two houses; in one it is the mother, a son, and a daughter, who are leprous, and these dwelt at the end of a long fjord, where there were lepers before, partly on the same farm and partly on the neighbouring farm; in the other house there is only one leper; these last dwell in another village, but the leper has constantly rowed during the fishery with his leprous cousin of the house first mentioned. Leprosy seems thus to be less attached to family than to intercourse.

In Olden, in Nordfjord, there live two families, and in Stryen one, who by marriage have become connected. In the latter and in one of the Olden families there is a leper in each; for the one case there have been three and for the other two previous healthy generations. In that part of Stryen where the one leper is, leprosy is relatively of frequent occurrence. I have visited the two Olden families, and I have seldom seen so fine well-grown and powerful people. A daughter of one of these families is leprous; she served at the house of a priest in Daviken, together with a lad who was obliged to leave his place on account of his leprosy, and who died shortly after. According to the usual custom among servants, she attended this man, made his bed, washed and mended his clothes. Also in these cases I must consider hereditary disposition as not very probable.

At the farm Bjorlo at Nordfjordeidet, which, like all farms here, lies on a terrace and is thus very dry, there have been two brothers and the son of one of them lepers; moreover, a more distantly related woman who dwells in the neighbourhood is also leprous. Here the relationship seems to be of importance, and likewise for the two children in another family. But no leprous ancestors are known for any of them. How the first leper at Bjorlo could possibly have got the disease it is not known, neither is any information to be had with regard to the more distantly related woman, but the second at Bjorlo has, during the building work, lain in the same bed with his leprous brother, and the son has naturally had intercourse with his leprous father,with whom he lived; and of the brother and sister, the sister was married with a leper, and the brother is brother-in-law to the two leprous brothers at Bjorlo, where he also dwelt and had intercourse with them. All the cases at Bjorlo, as well as the leprosy of the female relation in the vicinity, fall in the interval from 1843 to 1856.

Also here it seems that the locality and contact have equal weights in the scale with the family conditions.

The farm Möklebust at Nordfjordeidet is situated, like Bjorlo, on a terrace close to the sea. At this farm there have been, in the earlier part of the century, some lepers; descendants of this family are still living, some of them at Möklebust, and they are healthy. In the later time there have occurred three other cases, one in a family very well off, the only case known, and two likewise, the only cases, in another but poor family. These cannot be brought into connection with the other lepers at Möklebust, but in 1845-46 two of the leprous girls from Hjelmelandsbakken were servants at the farm; the one leper at Möklebust was at that time a young child, and had intercourse with the two leprous girls from Hjelmeland; ten years afterwards her own leprosy became evident, and eight years later the leprosy of her brother and of the third, Brite, was also manifest, she having been in intercourse with both.

Here are at least three cases of leprosy without any reasonable holding point for heredity, but with undoubted contact with other lepers.

I have registered the lepers who for a long time have been and still are at Aalfoten. In a bend of the fjord lie the houses of Shoreim in a crescent on the east side, and here may be observed the rare case that all the inhabitants belong to the same family; opposite on the other side lie the houses of Ise, which are occupied by different families, and in Aalfoten properly so-called, which lies round a little bight of the fjord, are situated Vik, Sigdestad, and in the innermost part Möklebust. If we now take the leprous cases only according to family relationship, we have only, out of fifteen, three cases which are without leprous relations. But if regard be had to the conditions of locality and habitation, we shall find in Shoreim six lepers in one family, and elsewhere eight (nine) lepers in six different families. One of them has changed his residence; but it is not known whether he was leprous before moving or not. Where the whole soil is occupied by one family, there leprosy is attached to that family; but where the farms and the various cultivations of the farms are managed by branches of different families, there leprosy extends over six families, while the number of cases is only three more; and it cannot be because the families are not numerous enough; some of them are as large as the Shoreim family, so that there seems to be quite material enough for heredity. But with heredity as explanation we do not come far enough, and we come incomparably further with contagion. At Thoreim contagion can only affect one family, and there we find many cases of relationship, enough to form what we may call a thoroughly leprous family. In the other farms we find many families exposed to contagion, and no less than six are affected. There is no want of more

direct indication of contagion for one case: Lars is the only leper in a large family, but he lived many years in the same house with his leprous father-in-law.

That leprosy is thus attached to the locality and not to the family, is still better shown by the following instance. In the parish of Bredheim, in Gloppen, there are three farms, Skreppen, Skinbo, and Sætre, situated high up in the hill on the sunny side and close to each other. Considered as a hamlet, they may be compared with Thoreim, but the conditions of habitation are quite different, for they are occupied by members of many different families, as is usual; for the families dwelling in the same homestead seldom intermarry; they marry usually with those who live a good way off. From these farms there are found eight lepers in five different households not related to each other; to these may yet be added three not noted here, each belonging to a separate house, in all eleven lepers in eight different families. Even if there may be found in other places leprous relations of these families, the circumstance would still be remarkable, but according to report there are no such leprous relations, and as regards two of the families, I have been able to verify the accuracy of the statement. If the conditions of habitation had been as at Thoreim, we should certainly in these farms have had fine family tables.

I can further name two similar cases, though not so strongly characterised. At the farm Reed, also in the parish of Bredheim, there has been leprosy in various families. In one there are not less than five cases. The family is large, and its branches are spread about in various places, but only at Reed is the family leprous. The oldest leper, John, lived with his brother David; the two daughters of the latter became leprous, and likewise a cousin born on another farm but reared in David's house, and finally another cousin who dwelt at Reed.

At Œstrem, in Gloppen, leprosy appears in five cases in three families. Two of the lepers have a leprous cousin on the mother's side; but as this cousin had a leprous uncle who dwelt in the same house, this relationship is not of much importance.

Considering that in a relatively very short time and in a rather limited territory I have been able to collect so many cases where the peculiar conditions of habitation and of the distribution of families seem to play an important part for the appreciation of the occurrence of leprosy, I must assume that this manner of criticising heredity must have its significance. It is here in the west country most usual that the several occupants on the same farm dwell together in one homestead, and it is just as usual that these occupants are not related to each other. The homesteads are usually remarkable for being extremely uncleanly, and their inhabitants are equally so. In the last few years a favorable change has begun to take place by repartition, as this generally involves removal from a

common homestead, and I have repeatedly had occasion to see that removal brings about a better domestic arrangement.

To trace the history of the occurrence of leprosy in the several districts in this country is nearly impossible. There are, however, some circumstances which indicate that the disease did not until quite lately extend to certain districts; this applies specially to Finmark, and here leprosy attacks, not only Norwegians, but also Ovans and Fins; here the lepers are not very frequently related. Now, it is only in the last decenniums that the traffic with these regions of the country has become more lively, and that the great fisheries in East Finmark have been frequented in a large scale by fishers from leprous districts of the country. This might indicate an importation of the disease. Also in Sondfjord there is a locality where leprosy does not seem to be of old standing, namely, Jölster; the number of lepers is small, and only few of them are related; most of the lepers here have served or rowed during the fishery in and from the strongly leprous districts near the sea. Nay, in one of these latter, namely, Nöstdal, it is maintained that leprosy is not of older date than from about 1820. I received in that respect completely similar accounts from several old people in the various parts of the valley. The first leper in the valley is said to have been a woman from the neighbouring village, Solliden, who moved into the farm Indre-Koame; the second was a woman from the neighbouring farm Koame, who moved into the upper part of the valley; the next two cases occurred again on the farm Indre-Koame. If we could depend on this account it would indicate an importation; the people who gave the information were more than seventy years old, and the tradition was similar in the upper part of the valley, in the middle, and in the lower part down near the sea. Now, Nöstdal is, or was a short time ago, one of the most leprous districts; there are only a few farms in the thickly populated valley that have escaped the disease. The habitation is the same as usual; the houses are collected for the most part in homesteads; those who dwell in the middle of the valley intermarry mostly with those who dwell in the higher or lower parts, and *vice versâ;* this produces in the course of time such a combination of families that in the short time I could dispose of, it was impossible for me to attempt to get a clear account of them, as the valley contains between 3000 and 4000 inhabitants. Nevertheless, it appears, according to the information we possess, that the number of lepers without any leprous relatives is not so little (about one fifth). If, now, leprosy is contagious, then both the circumstance that there may be found strongly leprous families, and the circumstance that there may be found other families in which only a few or single members are leprous, will find a natural explanation. For if leprosy comes, as here, into nearly every homestead in the valley, it may in different homesteads affect the same

families, and in the same homesteads affect several different families, just as we have seen in the farms Skiuls, Skreppe, and Sætre, in Nordfjord. When the families are very much intermingled, it can excite no wonder that we have more family cases than isolated cases. With heredity as explanation these latter cases become quite enigmatical, while on the hypothesis of contagion the family cases will, on the contrary, not only not be surprising, but will be very easily intelligible. I shall, in further demonstration of this, now describe what may be observed in Tresfjord, in Romsdal.

I have there found three powerful races issuing from the three farms Skjærsvold, Eidhammer, and Bradstad, and the members of which are found spread round about the fjord and partly beyond. I have noted down more than 300 members, but several branches are not specified because no leprosy is found in them. The tables extend a good way into the former century; great-great-grand children of the eldest noted are now mostly middle-aged, and some of them old persons. A man of the eldest generation was married for the second time with his first wife's sister's daughter, and from this marriage there are now great-great-grandchildren, who will soon be marriageable. If we reckon in the members of these races not registered, we may probably calculate between 600 and 700, among whom there have occurred fourteen cases of leprosy. I do not think any case has escaped my notice, as the old woman (above 70) from whom I have most of the names indicated, had about the 300 names in her memory, and the accuracy of her memory I have been able to confirm by inquiries elsewhere as regards a great part of the information given. In spite of the extent of the races there are relatively very few interior marriages, and there are thus a great many importations by marriage; only for one family I have been able to find leprosy which might be brought into connection with two such importations, but that this connection can have any influence on the bringing in of any hereditary disposition is very doubtful, as these lepers, who were outside of the great races, all except one dwelled on the farm Vike, where four of the fourteen cases of leprosy had occurred within the races, and in four different families, of which three are related nearly, and the fourth more distantly. If we now follow the subject on which we have entered, namely, that of local circumstances, we shall find, besides the four in one farm, three more children of common parents in one farm, two in another, and two nearly related in two neighbouring farms; these two have had intercourse together, and in the house of one of them a leprous man has been servant and had his bed and clothes attended to by the party concerned.

There remain thus only three isolated cases; two of these are of ancient date, about 1830; on one farm, where the third case occurred, there has been leprosy before, and there is still one leper.

Leprosy occurs thus neither much diffused in the many families of these races, nor in many places in proportion to the diffusion of the races. The proportion is not very different from the proportion between the lepers and the inhabitants, otherwise this cannot be determined accurately, as it is not possible to obtain such accurate information about lepers from former times. Moreover, the same tradition exists in Tresfjord as in Nöstdal, that it is not more than fifty or sixty years since leprosy came into the fjord.

We have for these races as good probability as in general can be obtained for there not having been leprous in previous generations, and we find nowhere parents and children simultaneously lepers. In regard to the very irregular heredity, and to the presumption necessary for heredity of there having been leprous ancestors in the previous century, since neither with respect to these races nor indeed to any other can the slightest probability of progressive degeneration or anything like it be shown by any one, this occurrence of leprosy appears susceptible of natural explanation by help of contagion. While these three races are spread everywhere round the fjord, no contagious disease which affects a greater number of the farms situated here can well avoid affecting members of these races. The contrary would, indeed, be remarkable. Such an interpretation is also supported by the appearance of cases in groups in races which can by no means be designated as thoroughly leprous; indeed, such races can scarcely be said to exist; very leprous races are extremely rare, while, on the contrary, the more limited family may be so, and then it is most usual that several brothers and sisters, chiefly those of more nearly the same age, are the lepers. Of this there are three instances in these races, and I have before shown by instances that such may find explanation by help of the habitation of the family and contagion, while the other families of the same line may be quite exempt. The circumstances in Tresfjord do not otherwise offer anything particularly noteworthy; the lepers of whom I have been able to get an account, are partly the only ones of the family, partly there are only a few in the same family; the same case as elsewhere, where the families are not large or leprosy not very prevalent.

I could yet make commentaries on some of the cases collected, but partly these would be incomplete and partly they could not give us any new points of view. I will only remark that I cannot point out any case in which heredity has any preponderating probability for it, and I can scarcely be accused of having in the instances given omitted such as according to current notions might seem to have the presumption of heredity on their side; on the other hand, I will not omit to point out that, for an incomparably preponderating number of cases, there is wanting all reasonable indication of direct occasion for contagion, which appears also from no mention

being made of the same, as it must be supposed that I would have produced whatever evidence I could produce in that direction. If, however, we now take a review of what has been adduced, we shall find that such direct indications are not totally wanting, and I will specially point out the two importations from non-leprous districts; further, it is shown by instances that certain conditions in the occurrence of leprosy receive their best explanation from contagion, and thereby also some of those cases which otherwise are usually considered without further reflexion as hereditary, become subject to a criticism from which they have hitherto been exempt. I think that I have hereby given at least a beginning for a real indication of the much disputed foci of leprosy, and of the occurrence of leprosy also in families being possibly independent of relationship. If we now compare herewith what we found in Bergen, it appears to me that the probability for contagion rises little by little, while the probability in favour of heredity sinks in a corresponding degree.

As, however, nothing yet has been produced which could be strictly called a proof, we must seek after other things which possibly might confirm or controvert what has hitherto been adduced, and I shall therefore endeavour to extract from the statistics of leprosy in its new form what may be serviceable for this purpose.

These statistics, for the compilation of which we are indebted to Mr. Inspector Hartwig's detailed lists of all the lepers in the country, made voluntarily on his own account by reason of the interest which he has taken in the matter, while the tedious elaboration of the same is also his work, are based on the assumption that all the lepers, so far as is practicable, are registered in that year wherein their disease is supposed to have originated. As nearly all the indications of the duration of the disease are based on the patient's own statement, and this statement, as we have seen, is by no means trustworthy, there will, of course, be many errors in respect of the patients being registered, most probably, years later than the time when their disease really began. For the sake of comparison between the single years, and especially between the periods of five years, this error will probably not be of any great importance, as it will be most likely tolerably equally distributed. The error affects the essential part of the tables, namely, the number of new lepers received yearly; but if one endeavours to correct it by taking a corresponding number of new arrivals for each quinquennium, and carrying it back to the previous quinquennium, the chief results will be the same, only the diminution of new cases which appears everywhere will be more manifest. After many attempts to combine the statements in various manners, I have fixed on the following as the most comprehensive. The districts are

arranged with regard to the asylums to which the largest number of patients are admitted. These asylums are Reitgjárdet near Trondheim, Reknás near Molor, St. Jorgen; the Asylum No. 1, and Lungegaard's hospital near Bergen.

Provinces of Tromsö and Throndhjem Reitgjærdet.

Series of years.	New cases.	Died.	Died in percentage of all lepers.	Placed in asylums.	Placed in asylums in percentage of all lepers.	Remaining.	Year.
1851—1855	304	...	...	...	...	722	1856
1856—1860	348	261	26	23	2·3	700	1860
1861—1865	349	177	17	293	28·0	559	1865
1866—1870	290	155	18	186	21·9	484	1870

Nodre and Sondre Nordmore, Surendal and Sundal Reitgjærdet and Reknæs.

1851—1855	54	...	...	...	...	107	1856
1856—1860	80	32	18	14	8·2	121	1860
1861—1865	86	43	20	45	21·2	117	1865
1866—1870	82	40	24	43	21·5	110	1870

Indre- and Yltre- Romsdal, Ostre-, Nordre-, Vestre-, Indre-Sondmore Reknæs.

1851—1855	87	...	...	...	...	175	1856
1856—1860	101	33	13	28	10·9	190	1860
1861—1865	72	34	13	94	35·9	130	1865
1866—1870	61	35	13	50	26·1	102	1870

Yltre- and Indre- Nordfjord and Nordfjordeidet, Reknæs and Bergen Asylums.

1851—1855	43	...	...	...	...	100	1856
1856—1860	53	19	13	47	33·3	74	1860
1861—1865	42	17	14	19	16·3	78	1865
1866—1870	32	24	22	19	17·2	65	1870

Kinn, Yltre- and Indre- Sondfjord Bergen Asylums and Reknæs.

1851—1855	183	...	...	...	...	433	1856
1856—1860	209	83	13	211	35·0	305	1860
1861—1865	153	61	13	144	31·4	246	1865
1866—1870	112	52	14	137	38·2	168	1870

Yltre-, Midtre-, Indre-Sogn and Lærdal Bergen Asylum.

Series of years.	New cases.	Died.	Died in percentage of all lepers.	Placed in asylums.	Placed in asylums in percentage of all lepers.	Remaining.	Year.
1851—1855	128	...	...	...	...	319	1856
1856—1860	83	58	15	117	30·5	205	1860
1861—1865	61	48	18	45	16·9	161	1865
1866—1870	47	30	14	43	20·6	127	1870

The Town of Bergen.

1851—1855	8	...	...	...	...	23	1856
1856—1860	12	1	3	4	11	9	1860
1861—1865	11	1	5	6	30	12	1865
1866—1870	5	2	11	11	64	15	1870

Sondre Bergenhus District.—Bergen Asylums.

1851—1855	195	...	...	...	...	467	1856
1856—1860	173	113	18·5	115	19·0	366	1860
1861—1865	143	98	19·0	71	14·0	337	1865
1866—1870	124	97	21·0	80	17·3	281	1870

Stavanger, Lister, and Mandals Districts.—Lungegaard's Hospital.

1851—1855	77	...	...	...	...	231	1856
1856—1860	66	58	20·0	30	10·6	194	1860
1861—1865	69	55	21·0	16	6·0	189	1865
1866—1870	35	49	21·5	11	5·0	157	1870

Nedenæs, Buskerud, Kristians, Hedemarkens, and Akershus Districts.

1851—1855	22	...	...	...	...	51	1856
1856—1860	6	11	19·0	...	...	44	1860
1861—1865	12	16	28·5	3	6·5	36	1865
1866—1870	9	16	35·5	4	9·0	24	1870

The quinquennium 1851 to 1855, in which only the old St. Jurgens Hospital, Reknæs and Lungegaard's hospital were in operation, has been included, with regard to its new cases; as we may suppose that the statements from later years, after the beginning of the regular counting in 1856, comprise approximately all

the new cases observed in those years. For former years we have not so trustworthy returns as to be able to judge with certainty whether leprosy was on the increase before 1850 or not. In 1836 the number is said to have been 659; in 1845, 1122; and in 1856 the number was, according to what we now know, about 2800. There appears, therefore, to have been a very strong increase; but if we consider how many cases are even now, when the counting is executed by the medical men, overlooked or unknown from one year to another, the number of cases overlooked when regular countings were not instituted, and the countings such as they were depended on the work of non-medical people, must have been still greater. However, I must with Bidenkap assume that the number cannot have been so great as the difference between the figures stated, and that therefore an increase in the number of lepers has really taken place in the present century. This agrees also with the tradition found in many places, and which I have mentioned above. What we may assert with greater confidence is that the numbers in the two quinquenniums 1851—55, and 1856—60 have remained nearly unaltered. The number of new cases, at least, were in these two periods about the same, namely, respectively 1101 and 1131 for the whole kingdom, or about 220 new cases yearly; but in the period 1861—65, 998, or about 190 yearly; and in 1866—70, 797, or about 160 yearly. Since 1860 there is therefore a decrease in the number of new cases, which appears to be steady and not a chance fluctuation. The total number has also decreased in corresponding proportion; in this table there are only noted as remaining those who are at home in the districts, in order to show in what degree the districts are emptied by the receptions into the asylums. As now the number of those remaining is influenced by removal, as also by immigration and erasure from the lists, these numbers are not so trustworthy to guide us as those of the new cases. It should be remarked that the number of those remaining at the end of 1855 is not known, for which reason it was necessary to give it at the end of 1856, and this makes some confusion in the quinquennial periods, but it could not be avoided

It must be our task, if possible, to find the cause of this decrease, and specially to call attention to the new cases. In looking at the tables, the groups of which are formed with reference to the respective asylums, we shall find that these stand very differently in the different districts. In the provincial districts of Sogn, Sondre, Bergenhus and Stavanger, the number of new cases was greater in the first quinquennium than in any of those following; Sogn especially takes a remarkable place in this respect. If we compare the decrease afterwards observable with the number of those placed in the asylums, it does not seem to have had much influence on the decrease of new cases which was already in progress. Neither is the

number of deceases in these dictricts much greater than in others; only in Söndfjord is it in any considerable degree less.

In Nordmöre the number of new cases has remained at about the same point, and is in all the last three periods of five years even considerably larger than in the first, notwithstanding that the admissions to the institutions, have been great in the two last quinquenniums. If the admission into the asylums had had any influence on the number of new cases, we might have expected a decrease in the last quinquennium, as the greatest number of admissions began in the last but one. But, as may be seen from the table, the receptions from this district have not been sufficient to empty the district in any great degree.

The same applies to Romsdal, Söndmöre, and Nordfjord; the diminution in the number of new cases seems here not to have taken place until they began to be received into the asylums, and this diminution is about in the same proportion as elsewhere, but the numbers are so small that scarcely any inference can be drawn therefrom.

This seems, on the contrary, to be the case at Söndfjord. Here is increase in the number of new cases from the first to the second quinquennium; in this second period 35 per cent. of the lepers of the district were placed in the asylums, and in the third quinquennium the number of new cases is diminished by 46; in this period 31·4 per cent. were placed in asylums, and in the fourth quinquennium the new cases were fewer by 41. In no district has the evacuation been so complete as here since the asylums began to receive patients, and till 1870, or in fifteen years, the number of lepers has diminished in the home district from 433 to 168.

In the provinces of Tromsö and Throndhjein the admissions to the institutions seem to have been of importance. The number of new cases increases from the first to the second quinquennium; in the latter period there were placed in the asylums only 2·3 per cent. of the lepers, and in the third period the number of new cases is the same as in the second. But in the third quinquennium there were placed 293 lepers, or 28 per cent., and in the fourth there are 59 fewer new cases.

In order to get larger numbers we may arrange the districts in two large groups, namely, those from which the reception in the asylums began on a larger scale in 1856, and those from which the reception did not begin until 1860. We thus get the following comparison with respect to the number of new cases in the same four quinquenniums:

New cases in—	1851-55.	1856-60.	1861-65.	1861-70.
Province of Bergen and district		−38	−117	−124
of Stavanger	634	596	479	355
Other parts of the country	467	533	519	442
		−68	−16	−77

Number in the district was—	1856.	1860.	1865.	1870.
Province of Bergen and district of Stavanger	1573	1153	1023	915
Other parts of the country	1055	1055	842	720

Admitted to the asylums:	1856-60.	1861-65.	1866-70.
Province of Bergen and district of Stavanger	524 (24 p. ct.)	301 (18 p. ct.)	301 (22 p. ct.)
Other parts of the country	65 (4 p. ct.)	335 (21 p. ct.)	283 (22 p. ct.)

It is difficult to doubt, according to these figures, that the evacuation in the districts by reception into the asylums has an influence in diminishing the number of new cases; although it may be assumed also, especially in regard to single districts, that this is not the only factor. But before we can pass a final judgment in the matter we must await the experience of future years.

But, if we now assume that the reception into the asylums or isolation has contributed to the decrease of leprosy, it will be of interest to discuss why isolation should operate in that direction. Whether the disease is hereditary or contagious, isolation must be useful; for even if we assume an hereditary abnormity, which may become leprosy without leprous ancestors, yet those who are manifestly leprous must be supposed to transmit the abnormity in a stronger degree than those who are only about to acquire it.

It must therefore be expedient to prevent as great a number of manifest lepers as possible from propagating. The question then arises whether it is possible that the isolation during the short time of fifteen years should have been able to exercise influence on the number of hereditary cases. This seems to be a question which may without any further commentary be answered negatively, especially as the good effects of isolation appear already in the quinquennium immediately following. Such a state of the case is, however, quite consistent with contagion; and if we take up the official statistics we shall find that the decrease does not begin in the year after the commencement of isolation, and not until four or five years afterwards, and this corresponds with the assumption that the patients usually date the commencement of the disease too late. I shall endeavour to elucidate this by an imaginary instance. I suppose that the number of lepers in a district has been for a long series of years uniformly the same, for instance 100, and that there have been constantly 10 new cases yearly and 10 deaths. Of the 100 alive every year, 10 patients infect 10 new ones, and the latter do not become aware of the dis-

ease until five years later. If we set this in the form of a table we have—

For 100 lepers, for instance, in		1851—10 new cases in		1856
„	100	„ „	1852—10	„	1857
„	100	. „	1853—10	„	1858
„	100	„ „	1854—10	„	1859
„	100	„ „	1856—10	„	1860
			50		

Now, we begin to isolate in 1856, so that I have still in 1856—

For	100 lepers in	1856—10	new cases in		1861
But	90	„	1857— 9	„	1862
„	80	„	1858— 8	„	1863
„	70	„	1859— 7	„	1864
„	60	„	1860— 6	„	1865
			40		

By our isolation we cannot effect any diminution in the number of new cases in 1856—60, because the new cases in these years are really attributable to the previous quinquennium, but the effect does not come to light until the following period, 1861—65. The instance is not quite imaginary. We can suppose the number of lepers existing at the end of 1856, with a proportional addition for the different districts, to represent with tolerable accuracy the average number of the lepers living in each district in the period 1851—55, and likewise the number living at the end of each following quinquennium, with a proportional addition, to represent the average number of the quinquennium; and we find then the number of new cases in each period in several districts standing in a tolerably steady proportion to the number of lepers living at the end of the previous period; and the proportion to the average number will be approximately the same. We have thus in the provinces of Tromsi and Throndjein—

The number at the end of	1856=722	and 348	new cases in	1856-60
„ „	1860=700	„ 349	„	1861-65
„ „	1865=559	„ 290	„	1866-70

add for Söndfjord—

The number at the end of	1856=433	„ 209	„	1856-60
„ „	1860=305	„ 153	„	1861-65
„ „	1865=246	„ 112	„	1866-70

and for the whole kingdom—

The number at the end of	1856=2628	„ 1131	„	1856-60
„ „	1860=2208	„ 998	„	1861-65
„ „	1865=1865	„ 797	„	1866-70

As, now the average number will be higher than that noted at the end of the quinquennium, the proportion will be that there

will come about 8 per cent. new cases annually from the lepers living annually in the previous quinquennium. In my instance I took for the sake of convenience 10 per cent. In some districts there are deviations from this proportion; in some it is over and in others under 8 per cent., but the larger the numbers we work with the nearer we come to this percentage. It may, of course, here also be objected that the time for observation is too short, and I do not wish from these three periods of five years to argue for any regular law; but it seems to me not unreasonable from the number of new cases in 1851—55, which is almost accurately the same as in 1856—60, to conclude that the total number of lepers in the kingdom in the period 1846—50 was about the same as in the subsequent quinquennium. The sudden decrease from 1861 would then be of so much more importance; and I for my part am very much inclined to make the prognosis of the future course of leprosy the subject of a simple arithmetical calculation; time must show whether such a determination would be precipitate or not.

Although there seem to be rather positive indications that isolation has contributed essentially to the undoubted decrease of the new cases, and although such an effect of isolation cannot possibly be made to support the notion of heredity, but must, on the other hand, furnish a strong argument for contagion, I will not omit to repeat that, by reason of the apparent exceptions observable, for which I can find no satisfactory explanation, I will not attempt to pass any decisive judgment in this question. I shall only insist on one thing. If leprosy is contagious it would be more probable that the tubercular form, with its more abundant productions and more frequent ulceration, should be more dangerous than the smooth form; and, therefore, in order that the isolation should work most favorably, the tubercular patients should be specially isolated, and preferably as early as possible. The first part of this condition is fulfilled by a preponderatingly great number of tubercular patients being admitted into the asylums. Of those placed in the asylum No. 1 there are no fewer than 631 tubercular against 193 of the smooth form.

The second part of the condition is not so well fulfilled; most of the patients not being admitted until several, some times many, years after the evident breaking out of the disease. But their home is at least generally very well evacuated. Of those patients received into the asylum there are only 62 leprous children known, and of these no less than 57 are received into the asylum.

We will now assume that the asylums have been without any influence on the diminution, and endeavour to discover whether any reasonable cause for the same can be found outside of them. Just as there might be persons of opinion that the cause of the descendants of the lepers or leprous races who immigrated to Bergen not

being leprous is that the hereditary disposition is modified by altered conditions of life, so there might also exist the opinion that circumstances during the last fifteen to twenty years round about in the country districts have been so much improved as to counteract the hereditary disposition. Against this view there is first the circumstance that the proportion of family cases seems to be still the same as before, while they ought to be proportionally more numerous. For if the conditions of life should be able to counteract the hereditary disposition, then they must also counteract the occurrence of the so-called spontaneous cases, and as regards these must have incomparably better chance for victory than in combating an hereditary disposition. In the next place, I think that it would be very difficult to point out any essential change in the conditions of life until, perhaps, after 1870, at least in the provinces of Bergen, Romsdal, and Nordmore. How it may be in other districts I have no definite idea. The manner and conditions of life are never in the same place changed all at once, while circumstances demand a slow development; now, there is certainly in the west country a manifest development and improvement of all conditions, but it is, according both to my own observation and to the unanimous testimony of well-informed people, very slow. And according to experience, a long time is required—many generations—for an hereditary peculiarity to disappear. From whatever side we consider the case, it appears that heredity can have no influence on the decrease of leprosy, which may be most reasonably accounted for by its not existing at all.

I have now produced what, according to the extent of my investigations, I have been able to collect in the way of information as to the occurrence of leprosy here in this country, and its conditions, and I have endeavoured to point out those things which appear to me to be influential for the guidance of our judgment. Even if I have not been able to furnish any decisive proof in any direction, I think that I have pointed out a number of phenomena in the occurrence of the disease which find a natural explanation by supposing contagion, but which, on the contrary, must remain unexplained under the supposition of heredity. Leprosy will thus, according to my conception, come into the category of specific diseases which are contagious, but, like specific diseases in general, are not transmitted by inheritance.

But if leprosy is a specific and contagious disease we might, perhaps, expect to find that, like syphilis, it is also transmissible to the offspring. That such is the case can neither be denied nor affirmed. There are some observations of leprosy in so early an age, in the first and second year of children born of leprous parents, that they might be supposed, by reason of the slow development of this disease, to have got the disease while in the uterus. But the cases

are so extremely rare, and the leprous parents who have children are so many, that it is not probable. Most of the children who have leprosy are over five years of age, and it seems, indeed, reasonable to suppose that they have contracted the disease after birth. What might be most likely to direct our thoughts to a transmission of the disease to the ova is the fact that the testicles, according to my later investigations, are always leprous, and, as it appears, from the very beginning of the disease. In a patient who took service with a leprous master and became leprous the year after, one year later was placed in the Lungegaard Hospital, and died there half a year afterwards, it was found that, besides the skin, the liver, the spleen, the nerves and the testicles were leprous, and I was able to demonstrate this to Dr. V. Carter, from Bombay, who was just then residing in Bergen, to study our arrangements with respect to leprosy. Now, the products of the leprous affection in the testicles are found not only in the intertubular connecting tissue, but also in the seminal canals. As to this point I was for some time in doubt, but I have now obtained preparations in which it appears quite unmistakably; for the retrograde elements may lie in rows in the seminal canals, and by their magnitude partially enlarge them, so that a canal isolated for any considerable length takes the appearance of a string of small beads. I have not yet found any corresponding affection of the ovaries. It is now sufficiently ascertained that leprous men can procreate children; and when leprosy attacks the testicles in the manner mentioned, it may easily be supposed possible that the leprous contagion may go together with the spermatozoa. This anatomical discovery in leprous testicles gave me the idea that the syphilitic orchitis might perhaps have some influence on the transmission of syphilis to the offspring. With reference to leprosy nothing certain can be said, as we have seen.

The theory of leprosy being a contagious disease, and not hereditary, has been brought forward, in 1869, by Boynat-Landré, in his book 'La Contagion seule cause de la Lepre.' However, we differ in our views respecting heredity. While Landré quotes the discrepancy between the transmission to offspring of syphilis and the heredity claimed for leprosy as an argument against the latter, this discrepancy, in my opinion, might just be adduced as strengthening the notion of the heredity of leprosy. On the other hand, there are parts in Landré's book the importance of which can scarcely be shaken, and which have hitherto met with no opposition namely, the history of the course of leprosy in Surinam, and the cases related of leprosy in descendants from European parents. Landré's representation stands to me in these respects as the most convincing evidence hitherto given of the contagious nature of leprosy.

While leprosy may be thus indirectly proved to be a specific

disease by demonstrating its contagiousness, it would, of course, be the best if a direct proof could be given. I will briefly mention what seems to indicate, that such proof is, perhaps, attainable. There are to be found in every leprous tubercle extirpated from a living individual—and I have examined a great number of them—small staff-like bodies, much resembling bacteria, lying within the cells; not in all, but in many of them. Though unable to discover any difference between these bodies and true bacteria, I will not venture to declare them to be actually identical. Further, while it seems evident that these low forms of organic life engender some of the most acute infectious diseases, the attributing of the origin of such a chronic disease as leprosy to the apparently same matter must, of course, be attended with still greater doubts. It is worthy of notice, however, that the large brown elements found in all leprous proliferations in advanced stages, of which I have in 1869 already given engravings, republished in 'Leprous Diseases of the Eye,' by O. B. Bull and G. A. Hansen, bear a striking likeness to bacteria in certain states of development, as these are represented by Klebs in the first number of 'Zeitschrift für Experimentelle Pathologie und Pharmacologie;' and further, that in almost every preparation from a leprous tubercle, made with the utmost care to avoid contamination and kept for a number of days in the damp chamber, are developed conglobate masses of spherical bacteria or zooglœa. It would be desirable that other inquirers should direct their researches to this point.

I could further point out several features in the pathology of leprosy that tell strongly in proof of its specific nature, but I must desist from doing so in this article. Perhaps I may make it the subject of a further communication.

Chronicle of Medical Science.

REPORT ON PATHOLOGY AND THE PRACTICE OF MEDICINE.

By John T. Arlidge, M.D., A.B. Lond., F.R.C.P. Lond.,
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On Warm Baths.—Dr. Lasègue has endeavoured, by experiment and observation, to place the use of warm baths on a surer and more rational footing, believing that it may be systematised after the same manner as that of cold water baths. In seeking to estimate their value he excludes mineralized waters and the results of hot air and vapour, and he insists on the rigid determination of temperature by the thermometer, and this not only at the time of immersion, but throughout the duration of the bath. In the case of the cold bath there are three essential and definite conditions, viz., pre-action, action, and re-action; but in that of the hot bath only one factor is of consequence, viz., that of action. For the therapeutical effects of the latter are not modified by the previous temperature of the patient, whether raised by exercise or artificially, or whether reduced by a cold atmosphere. The only observation requiring notice in respect to this point is, that after the night's rest in a uniform temperature, there is an enhanced susceptibility to agents acting on the skin, alike in the case of cold and of warm baths.

When a metallic bath is employed there is a loss of 3·70 degrees (Fahr.) in the space of fifteen minutes; such rapid cooling must, in no small degree, affect the results of a bath, and M. Lasègue is convinced that to secure good results no reduction of temperature must be permitted. Experiment will prove that, when the temperature is maintained at the same point throughout the bath, the effect will be calmative and agreeable, but that, on the contrary, when cooling is allowed to progress unimpeded, a feeling of fatigue and malaise will last for several hours afterwards. Such drawback may matter little in the case of healthy people, but in that of the sick is of material moment, and M. Lasègue would not sanction the ad-

ministration of a bath, with decreasing temperature, to a patient labouring with cardiac disease, especially with lesion of the aortic orifice. On the other hand, when the heat has been kept up, he has found cardiac cases signally relieved, and consumptive patients experience beneficial sedative effects.

As to the effect of elevating the initial temperature of the bath, taken at 93° or 95° (Fahr.). The highest supportable degree may be assumed as 116°; but this cannot be borne generally without suffering, and more particularly so if submitted to forthwith. Moreover, it presents no therapeutical value. The same may be said of the plan of giving a series of baths, beginning with tepid and advancing progressively to the highest admissible temperature, for the apparent purpose of gradually accustoming the patient to the employment of hot baths. Each bath, on the contrary, should be regarded therapeutically as complete in itself; and a series as so many repetitions of the same agent; and the indications for its use must be determined by what may be termed its calorific evolution, or its whole action from beginning to end.

The duration of a bath is on an average twenty minutes, and can rarely be extended with advantage beyond that time; and the elevation of temperature thought desirable should be gradually obtained, taking care that the maximum be reached within the last five minutes, and be not farther prolonged. The rate of elevation may be very slow at first, and more rapid towards the close, or may be carried on uniformly at about three to four degrees every five minutes.

The modifications in effect produced by prolonged immersion are less evident in the instance of hot baths than of cold; but it is certain that their value is not co-adequate with their continuance. It is the traditionary doctrine that a hot bath is stifling, that it accelerates the respiration and circulation, and is, consequently, inimical to those whose heart and lungs are unsound; but these evils are found to be imaginary if the bath be administered in the only rational manner of progressive heating.

The general lessons arrived at are:—that a bath shall not be prolonged above twenty, or at most thirty minutes; that the temperature on entrance shall be lower than that of exit, whatever be the extremes; that the augmentation of temperature be progressive and without shocks; that the useful maximum is 113° and seldom 116°, and is readily tolerated, provided the sensations produced by the contact of steam on the parts of the body not submerged be obviated, and the maximum heat be not continued over eight or ten minutes.

On quitting the bath and taking to bed the patient quickly regains, not indeed his real temperature, which has somewhat altered, but an apparently natural temperature, in which he feels neither cold nor hot. Usually a transient perspiration ensues, but M. Lasègue leaves the patient alone, attempting neither to prolong nor abridge the secondary effects of the bath.

His experiments with cold ablutions after very hot baths have

convinced him that, unlike what happens after vapour baths, no grateful sensations and no benefit follow their use.

The action of a hot bath is twofold—physiological and therapeutical. The former is limited by the tolerance on the part of the patient, or rather of the skin which affords the measure of susceptibility. The latter is defined solely by its power to modify morbid phenomena. And in citing diseases in which he has employed baths, he mentions rheumatic arthritis producing deformity of joints, and expresses his conviction that the efficacy of warm baths in this disease is but little affected one way or another by mineral matters dissolved in them; that hot water at from 104° to 115° does vastly more good than a sulphur bath at 85°, and that temperature, and not ingredients, is the main element. He employs highly heated baths in the treatment of the disease in question, every two days; persevering in the course for several months, or ceasing, and then resuming them according to circumstances, They may be tolerated during relapses and the activity of the malady, but their usefulness is found during the remissions. When only a small number of joints are affected, and symmetrically so, as commonly happens in men, the good effects of these hot baths are still more pronounced; not only limiting the morbid action, but extinguishing also the malady. But in cases of such limited disease experience shows that the local application of hot water cannot take the place of the general bath. Moreover, the pain and stiffness which so often persist after attacks of acute or of subacute rheumatism, are effctively treated by hot baths; but unhappily these agents cannot secure against relapses. Besides rheumatic affections, hot baths have succeeded in curing various abdominal disorders, and in particular some forms of chronic diarrhœa. Lasègue has also used hot baths with advantage in chronic bronchitis, but reserves the notice of his results in this and other morbid conditions to a future period.—*Archives Générales de Médecine*, November, 1874.

Changes in Muscular Tissue in the course of some Infectious Diseases.—Dr. Leo Popoff, of St. Petersburg, has prosecuted some interesting researches respecting the changes undergone by muscular tissue in several infectious diseases, comprising recurrent fever, "abdominal typhus," "exanthematic typhus," puerperal fever, and cholera. He has done the same in one case of pyæmia, in two of hydrophobia, and in one of "pneumonia fibrinosa" with hæmorrhage into the brain and the recti muscles of the abdomen. The muscles principally examined have been the recti abdominis, the diaphragm, the pectorals, and the adductores femoris.

The change most frequently observed has been that noticed by Virchow, and described either as a parenchymatous inflammation of the muscles, or as granular metamorphosis. Indeed, this condition was remarked in every case, although to a varied extent and degree of intensity in the different kinds of disease just enumerated. It was most pronounced in cholera and in puerperal fever, and prevailed often to a considerable extent in the several forms of fever, in which,

however, it was associated with another lesion, either less or more advanced than itself—viz., with waxy metamorphosis of the fibres. Indeed, this last change was pre-eminently marked in the fevers named, excepting typhus exanthematicus, in which it occurred in two of three cases, but only in patches. But in those two cases the peculiarity was the extravasation of blood among the fibres of the abdominal muscles. In all the other instances of disease examined the waxy metamorphosis was but slightly developed, and only so in its earlier phases.

One general fact that comes out from the observations made is, that whenever a pathological change was found in muscles it existed to a wider relative extent and in greater intensity in the diaphragm, whence the inference rises that this muscular structure is peculiarly predisposed to morbid changes in the course of infectious diseases —a circumstance which must exercise no inconsiderable influence upon the course and termination of those serious maladies.

The phenomena of proliferation of muscular fibre is very common, and, indeed, almost constantly found in some forms of infectious diseases. Both Virchow and Waldeyer detected in the granular metamorphosis of muscular fibres a process of multiplication of such fibres, and Dr. Popoff now confirms their observation, adding, however, that the process is much more prevalent in the waxy metamorphosis of muscular tissue.

Among Zenker's illustrations are examples of waxy transformation, which show the presence of cell structures in considerable abundance in the adjoining portions of muscular fibre, and even within the already transformed waxy substance. Zenker assumed these cell-elements to belong to the perimysium and to have escaped from tearing of the sarcolemma. O. Weber noticed the same circumstance in the vicinity of the waxy degenerated parts of the fibres of inflamed muscle; but as Zenker has shown, this new formation of cells is not confined to the parts contiguous to the degenerated waxy tissue, but occurs also within that transformed tissue itself, for although it looks homogeneous in structure, it may be most readily seen when the muscular substance is cut obliquely. The cell growth proceeds from proliferation of the nuclei of the muscular fibres or the muscular corpuscles, so-called, of M. Schultze. Besides waxy homogeneous fibres, others may be met with having a delicate granular appearance, whilst others looking completely waxy are swollen and increased in volume. These last, on being torn by needles, appear filled with cell-elements, conglomerated by the waxy matter. The expansion of the fibres is attended by an approximation of the transverse striæ, which at the same time become gradually obliterated.

The same act of proliferation is witnessed in the disintegrating process noticed in degenerated waxy fibres. The cells themselves are either of the same form and dimensions as the muscular nuclei, having one or two nuclear bodies, and more or fewer corpuscles, or they are larger than those nuclei, and may become by progressive growth considerably unlike them. Moreover, with the advance of the waxy metamorphosis their outline grows less distinct, and they

require the artificial coloration of the tissue to bring them into view. The progress of the waxy change involves them in a coat of protoplasm which itself undergoes the same transformation, and thus it happens that these muscular nuclei come to diverge more and more widely from their normal form and assume the characters of the structures described by Waldeyer as muscular cells. Cell proliferation usually proceeds simultaneously in the connective tissue and in the muscular fibres, and the building up of the latter from the former is a circumstance that cannot be admitted. The employment of polarized light proves that the changes in waxy degeneration are not so profound as the external aspect of the tissue would indicate. It likewise shows that waxy degeneration is not a more advanced stage of granular change, and that it is unaccompanied by any true chemical change of the muscular substance.

Popoff agrees with Waldeyer in viewing the so-called waxy metamorphosis as the result of inflammatory action, and the glistening aspect accompanying it, and to which it owes its name, he attributes to the same morbid process.

Lastly, Dr. Popoff notes the occurrence of changes in the blood-vessels of the diseased muscular tissue. Effusions of blood are common among the fibres, but stand in no constant relation with the extent of waxy metamorphosis. The principal changes in the vessels themselves (as seen in the several kinds of fever) partake of the character of inflammation, and are most pronounced in their middle and inner coats. The nuclei in the muscular coat undergo multiplication, and contain two or three nucleoli. At the same time the endothelium is thickened and its cell-elements swollen, so that the lumen of the vessels is much constricted; to this circumstance may be attributed the tendency to effusion and rupture. This inflammatory process of the blood-vessels is not of the nature of the chronic endo-arteritis observed by Virchow in old people, for it occurs equally in young subjects.

Ammoniacal Urine.—Prof. Gosselin, in conjunction with M. A. Robin, an hospital interne, has made an important addition to his paper on "Ammoniacal Urine and Urinary Fever," analysed in our last "Report on Medicine," by showing how the toxic and destructive effects of such urine may be relieved or overcome. Without following the writers of the paper before us in their review of attempts made to overcome the alkalinity of the urine by the administration of acids, and of the various acids, inorganic and organic, resorted to, we may at once state that the remedy now advocated from practical experience is benzoic acid. It is shown that this acid in its passage through the economy becomes by combination with nitrogenous material resolved into hippuric acid, and as such passes off in the urine. This transformation must be rapidly effected, inasmuch as hippuric acid appears in the urine in from thirty to fifty minutes after the ingestion of benzoic acid. Further, the weight of hippuric acid formed exceeds by one third that of the benzoic acid absorbed by the alimentary canal.

There exist, however, morbid conditions which arrest this metamorphosis; among such at present ascertained are jaundice, and articular rheumatism during its acute stage.

The hippuric acid generated by the administration of benzoic acid augments the acidity of healthy urine, and in the case of ammoniacal urine prevents the formation of phosphatic deposits by combining with the ammonia in the form of a very soluble hippurate of ammonia, whereby the ammonia is withdrawn from effecting combination with the phosphate of magnesia in the urine, and so forming an insoluble ammonio-magnesian phosphate.

Benzoic acid possesses the advantage of being very innocuous to the economy, but its insolubility in water is a drawback to its therapeutical employment. To facilitate its administration M. Gosselin gives it suspended in emulsion, or in syrup with some aromatic, as "limonade benzoique." He prefers the latter, and recommends the fluid warmed in order to aid its solubility, and also the addition of some tincture of canella, which as containing cinnamic acid, itself transformable into hippuric acid, adds to the efficacy of the dose. As a rule, it is best to begin with a gramme dose per day, and to rapidly augment the dose to three or four grammes. In many patients the medicine may be pushed to the extent of six grammes without inconvenience, unless, indeed, too long continued. An overdose is marked by dryness of the throat and a sensation of smarting. The result desired, viz. the reproduction of a due degree of acidity in the urine, or at least the neutralization of the ammoniacal condition, is not to be attained at once, but on an average requires seven to eight days. The extreme periods noted have been from five to nineteen days. The neutrality or returning acidity is always announced by a diminished tendency to phosphatic deposits, by decrease in the contained pus and blood, and by a progressive diminution of the fetor of the urine.

In some further comments upon the action of benzoic acid the writers express the opinion that, over and above the neutralization of the ammonia by the formation of a hippurate, the benzoic acid exerts a direct and healthy influence upon the inflamed mucous membrane of the bladder, and probably also retards the transformation of the urea and the consequent production of carbonate of ammonia.—*Archives Générales de Médecine*, November, 1874.

Lesions of Heart and Aorta in Variola.—M. Brouardel, physician to the Hôpital St. Antoine, has applied himself to show that in variola, especially when severe, there are certain definite lesions of the heart and aorta, over and above those already described in the muscular tissue itself of the heart, and that those lesions possess special characters. He notes the fact that such changes have been incidentally referred to by several French writers, and states that the basis of his researches was furnished by 389 cases of variola in a special smallpox hospital for females. Of the 389 treated, 87 died.

At the outset he makes a distinction between the vascular lesions

occurring at the early stage of the disease and those which happen towards its close, when suppuration, pneumonia, pleurisy, and articular rheumatism, may arise. The endo-pericarditis which complicates these secondary maladies does not differ from the cardiac inflammations ordinarily associated with articular rheumatism or with pleuro-pneumonia. He likewise detaches another group of cardiac inflammatory lesions met with in pregnant women, in those delivered and in those suckling children during the course of smallpox, in whom cardio-vascular changes may be attributed to the puerperal state. With respect to patients who survived, only those have been noted as suffering cardio-vascular changes in whom both the ear and sphygmograph have given evidence of such lesions.

Of the 389 variolous women, 53 presented the morbid changes in question, or nearly 1 in 7 of the total number. Moreover, 36 were delivered of children in the course of their illness, and of these 12 had vascular lesions. After deducting these cases and 5 others who had secondary rheumatic affections or pleuro-pneumonia, and in all of which vascular changes existed, there remain 348 of uncomplicated smallpox, of which 36 presented changes in the heart and aorta, verified in 27 instances by post-mortem examination. Analysis of the 27 autopsies shows that in 9 instances the aorta, the endocardium, and the pericardium, were affected ; in 5, the pericardium and aorta ; in 3, the endocardium and aorta, and in 1, the pericardium and endocardium. In no instance was the endocardium alone involved, but in 7 the aorta only was the seat of disease, and in 2 the pericardium only. In many instances recent false membranes were encountered on the pleura, attributable to the same pathogenetic condition. Endo-arteritis exhibited itself in the form of slightly raised patches of a bright or dusky red colour, known as gelatiniform patches. They were never found ulcerated. Although for the most part deeply coloured, some were of a rosy and even pallid hue. Their most common site in the aorta was just above the orifices of the coronary arteries. Microscopically examined, they are found to consist of a mass of flattened cells in the substance of the lining membrane of the vessel, and lying upon the middle coat, which presents little or no change. The external coat is likewise unaffected.

In the endocarditis of variola similar patches occurred, but not specially on the free margin of the valves, and, unlike the deposits of rheumatism, unattended by vegetations, or almost so. The pericarditis observed in cases that have died before the thirteenth or fourteenth day presents some special features. The false membranes formed are limited to particular spots, and recall by their form and size the appearance of pustules, although they are in reality small masses of fibrin and leucocytes. They occur especially in the auricles, and, next to these, on the anterior or posterior surface of the aorta and pulmonary artery. When death takes place at a later period of the disease these deposits are of greater magnitude, and are met with on the visceral and parietal surfaces of the pericardium. Pericardial effusion is small in amount, and serous only,

except when death happens late in the disease, when it is augmented in quantity and sero-purulent in character.

Myocarditis occurs, as a rule, in hæmorrhagic and confluent variola, and may exist apart from inflammation of the cardiac envelopes, although almost always concurrent with it. With reference to symptoms during life indicative of these cardio-vascular changes, M. Brouardel observes that none exist either sufficiently indicative of or that can be fairly interpreted as reliable signs of those changes. Precordial pain and anxiety, dyspnœa, and dulness on percussion, may, so far as can yet be made out, be affirmed as signs equally belonging to cases of smallpox without as with the lesions spoken of. Some help towards diagnosis is, however, supplied by auscultation and the sphygmograph, particularly when their results can be made to accord, although even aid so gained is apt to be nullified by the external eruption and the amount of complications that may exist. The predilection of the lesions in question for the valvular orifices, and especially for the aortic valves, has been noted already, but the amount of alteration in those localities is commonly so limited in extent as not to produce murmurs indicating narrowing or insufficiency, and, on the whole, when murmurs are present there is no certainty in the attempt to associate them with those particular cardiac changes. However, M. Brouardel surmises that when after two, three, or four days an aortic murmur (which when first heard is of a soft character) becomes harsher, and a second bruit is heard at the apex, recalling that of aortic insufficiency, we may still, with much misgiving, pronounce on the existence of the peculiar lesion he has investigated. With respect to sphygmography, he affirms that the lesion of the heart or aorta is first shown by a flattening of the apex of the line of ascension, and that this result attends on aortic insufficiency. The concordance of the sphygmographic and auscultatory signs he holds to be sufficient to predicate the alterations described; the presence of the latter by themselves is unreliable.

The cardio-vascular derangements commonly exhibit themselves at the outset of the eruption, progressively advance throughout the period of maturation, remain evident during desiccation and desquamation, and decrease and usually disappear during convalescence. The lesions have not the lasting character and remote consequences of rheumatic mischief in crippling the valves. Nevertheless, they are indications of the severity and gravity of the attack, the consequences of which they they do not seem materially to modify. Statistics show that they occur in a higher ratio in hæmorrhagic variola.—*Archives Générales de Médecine*, December, 1874, p. 641.

Scrofulous Angina.—Dr. Landrieux, the chief of the clinique of La Pitié Hospital, Paris, sketches the history and pathology of this severe form of scrofula, which was first described by Dr. Hamilton, of Dublin, in 1845. He quotes several writers, mostly French, who have contributed to elucidate the subject. The origin of the malady

is obscure; its advance insidious, unaccompanied by pain, and marked by no prefatory signs of inflammation of the mucous membrane of the pharynx and adjoining parts, which it especially seizes upon. Isambert considered the posterior wall of the pharynx to be usually the first point attacked, but Landrieux asserts this priority as most common in the soft palate and pillars of the fauces. Generally, when patients first consult their doctor, there are signs of previous mischief in the shape of cicatrices, adhesions, deformities, the pharyngeal lesions being at the same time in full operation. The appearance generally is that of induration with hypertrophy of tissues, shown by projecting, rounded rugæ of greater or less size, together with adhesions radiating in character and seriously interfering with the movements of the palate. The tonsils and adjacent mucous membrane frequently escape for a considerable time the morbid action, and this fact becomes somewhat diagnostic of this scrofulous disease from syphilis, which frequently attacks primarily those parts of the throat. The larynx is also slow in taking on the morbid process. According to Isambert, there is primarily hypertrophy of the mucous follicles of the affected membrane, soon followed by ulceration of their orifices. In the second period the mucous membrane looks pale and shrivelled, with vascular injections only here and there. But the characteristic features are the grey, dirty looking ulcerations, superficial only, and with irregular margins. The mucous membrane around them is not injected, and is devoid of the vascular contour around so frequently encountered in syphilitic eruptions of mucous membranes; it is almost impossible to scrape them clean, and they always maintain their caseous aspect. Although they show a preference for the posterior wall of the pharynx, they will at times invade surrounding parts; thus, by extending to the Eustachian tube they provoke exceedingly violent pain and cause complete deafness; or if they occupy the posterior or anterior surface of the epiglottis they give rise to various painful consequences; and, lastly, if they penetrate deeply they set up mischief in the bones, mostly at the level of the arch of the palate, but, as has happened, on the anterior surface of the cervical vertebræ. When cicatrization ensues, greater or less deformity of the soft parts is the result. It is a remarkable feature that, as is also usually the case in lupus of the face, the lymphatic glands are not in any degree involved by this ulcerative process. As a matter of course, the functional disturbances met with are regulated by the site of the disease. They consist chiefly in difficulty of deglutition and in loss of hearing. Commonly there is almost absence of pain; hence the frequency with which the earlier stages of the disease are overlooked. But later on, the act of swallowing is attended by much pain, and solid food has to be laid aside, whilst even liquids will not pass without great effort. In bad cases the arch of the palate is indurated and thickened and the pillars of the fauces destroyed, whence it happens that food is thrown forwards into the posterior nares, or the epiglottis is thickened and ulcerated and food drops into the larynx, provoking violent cough and often vomiting

or retching. The voice is frequently affected, although no primary lesion in the vocal cords is to be found. The alteration is especially in its "timbre," the voice is vibratile and nasal, and the emission of some sounds painful.

The course of the malady is habitually exceedingly gradual, and one of its most frequent modes of termination is by the onset of pulmonary phthisis. In the illustrative case recorded by Landrieux copious hæmorrhage of bright blood occurred on two occasions, the second attack, two days after the first one, proving fatal. At the autopsy this bleeding was found to proceed from an ulcerative opening into the external carotid. This termination is the least common of the three fatal kinds mentioned, viz. erysipelas of the pharynx, œdema of the glottis, and hæmorrhage. M. Constantine Paul asserts that three fourths of the cases are curable, but with this favorable opinion Landrieux cannot concur, and supposes M. Paul to have other forms of agina in view besides the severe variety described by Hamilton.

The diagnosis of this disease is attended by considerable difficulty. It is most likely to be confounded with ulcerative or gummatose tertiary syphilitic angina; but in this latter the ulcers have a different aspect, they discharge pus more freely, are deeper, are more threatening in aspect, and more rapidly induce periostitis. Moreover, syphilitic ulcers have sharper edges, which are injected, and have around them a red margin which readily bleeds; the pain attending them is more acute, their course more rapid; the lymphatic glands around become involved, and for the most part they are accompanied by special morbid phenomena, and often have their character made clear by the patient's history. Furthermore, the seat of syphilitic ulceration is especially about the soft palate or the larynx, whilst that of scrofulous angina is mostly in the pharynx. Yet it must be admitted that the diagnosis is at times impossible. This happens chiefly in scrofulous subjects who have contracted syphilis. To designate such cases M. Bucquoy proposed the term scrofulo-syphilitic angina.

Tubercular lesions of the mouth and throat are not likely to be confounded with this scrofulous angina, for their seat is different; they occupy the buccal walls and tongue, and almost always have no existence until the pulmonary disease has far advanced.

But there remains one other morbid condition from which scrofulous angina must be distinguished, viz. the tuberculous lepra or elephantiasis of the Greeks. In this malady there may be more or fewer tumours around the arch of palate, or on the pharynx, of considerable size, and either ulcerated or not; but the mucous membrane is of a violet-red hue and is anæsthetic, and in almost all instances the larynx is involved, and this usually from the commencement.

Lastly, cases of cancer of the throat may now and then be met with, particularly in persons somewhat advanced in life, which have to be diagnosed from the form of angina in question. In such instances the history must be inquired into, the primary seat of the lesion and its course determined, the presence of exalted or dimin-

ished sensibility and the condition of the lymphatic ganglions made out.

A review of the characters of the malady leads to its being regarded as a malignant ulcerative angina, of scrofulous origin, and to its pathological position alongside *lupus exedens* of the face. Otherwise it may be spoken of as a lesion identical with hypertrophic scrofulous or tubercular lupus.

The perusal of the case recorded by Landrieux will furnish the reader with the clearest conception of the history and course of this serious disease, and of the histological changes consequent upon it. —*Archives Générales de Médecine*, Dec., 1874, p. 660.

Pathology of Diphtheria.—Dr. Ludwig Letzerich has minutely examined the microscopical appearances of diphtheritic exudations and of the changes brought about in the various organs of the body where diphtheria has become a general disease. The contagious element of diphtheria he asserts to be a microscopical fungus, which is found to exist under four forms. The first form is that of masses or balls of microspores, of a dusky colour; their groundwork is hyaline and coloured blue by iodine and sulphuric acid; when detached, the minute corpuscles exhibit zigzag movements. To this form the observer assigns the name of bacteria-balls (Kugelbakterien). They very rapidly grow in size, and soon acquire the characters of the second form distinguished, having a waxy glistening aspect and strongly refracting light. These are called plasma-corpuscles. In a separate condition they are capable of movement and of amœoid contraction. Sooner or later micrococci develop themselves in the glistening protoplasm of these corpuscles, appearing as finely striated anastomosing processes radiating from one or more central or eccentric points. Later on small punctiform corpuscles form on these and completely occupy the interior of the plasma-balls, making their centres look black. These changed globules constitute the third form to which the name of micrococcus-globules is given. The rapid growth of the contained corpuscles soon stretches and at length bursts the enclosing envelope in dense masses or colonies of micrococci. These micrococci in their turn become bacteria-balls and go through the same series of changes. But under certain conditions the colonies of micrococci undergo a further transformation into a true fungus, which generates a Tilletia spore (*Tilletia diphtheritica*). In connection with these parasitic growths two forms of diphtheria are distinguishable, both pathologically and by experiment. The one form is that of local, the other that of general diphtheria; the former is a true exudative, ulcerating process; the latter, from the first, a process of infiltration followed by ulceration and exudation. At the same time it often happens that the one form is transformed into the other, and sometimes the two forms occur simultaneously in the same individual. The local form is the more frequent, but the general is always encountered in bad cases and in severe epidemics, and, in the nomenclature of some writers, constitutes typhoid diphtheria. The active agents in the production and spread of the disease in either form are the parasitic corpuscles above mentioned. When these bodies are

brought into contact with the mucous membrane they at once proceed with their cycle of development, and in so doing insinuate themselves between and beneath the epithelium, and thence penetrate into the mucous tissue and subjacent areolar tissue, and, when the tonsils are attacked, into the meshes of cells, everywhere leading to gradual breaking down of tissue by a species of ulcerative process. The blood-vessels themselves do not escape their ravages; they enter into and choke up the capillaries of the affected parts, and sometimes by the softening or erosion of tissue lay open the smaller blood-vessels and cause hæmorrhage. Moreover, the fact of the capillaries and larger vessels, becoming involved leads to lesions of nutrition, consequent on the cutting off of blood supply, or on the obstruction to the return of blood from the part affected, the lesions themselves occurring most readily in cell structures and their nearest allies.

Equally with the blood-vessels the lymphatics suffer, hence the glandular swellings so common in this disease. This penetration of the corpuscles into the channels both for lymph and blood explains the rapid diffusion of the disease throughout the body and the lesions complicating it of the several viscera of the body, foremost among which in the time and degree of invasion are the kidneys. But the spleen, the liver, and the muscular tissue of the heart, are in varying degrees also invaded by these fungus-corpuscles. The final consequence of the interpenetration of these parasites throughout the body is the generation, in a chemical point of view, of a morbid material or a decomposing agent, gifted with poisonous qualities—a true *contagium.*

Dr. Letzerich has confirmed the truth of the *rôle* of these parasitic corpuscles by direct experiments upon the lower animals.—*Virchow's Archiv für Pathologische Anatomie und Physiologie,* October, 1874, p. 457.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.,

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On the Therapeutic Uses of Phosphorus. By W. H. Broadbent, M.D.—Dr. Broadbent, in the present communication, records the successful employment of phosphorus in four cases, two being instances of anginoid pain, one of "essential or pernicious anæmia," and one of leucocythæmia. Dr. Broadbent is not yet able to define the forms of neuralgia in which the uses of phosphorus are specially indicated, though the most striking effects he has seen have been in cases of low nervous tone, in which the patient suffered from violent attacks of pain in various nerves. In one of the cases of anginoid pain here recorded the symptoms appeared to be due to aortic regurgitant disease, and the patient was taking tincture of perchloride of iron, with tincture of nux vomica and digitalis, and Dr. Broadbent gave a phosphorus capsule twice a day, after food, in addition to the other drugs. Very great improvement resulted from the phosphorus, to which Dr. Broadbent attributes the relief of the symptoms. In another case of anginoid pain, in which Dr. Broadbent could not discover any positively abnormal condition of the heart, aorta, or abdominal viscera, the administration of phosphorus was decidedly beneficial. He was led to employ phosphorus in the treatment of neuroses by his experience of the uses of arsenic in the same class of cases. The terms "pernicious anæmia" and "essential anæmia" (the latter so called by Trousseau) are applied to a malady which seems to Dr. Broadbent to be allied to Addison's disease, but differing from it in the circumstance that there is no bronzing of the skin and no disease of the supra-renal capsules. It is a disease which advances to an inevitably fatal termination, and the immediate cause of death is the want of blood, but the affection itself seems to Dr. Broadbent to be caused by the undue waste or the imperfect renewal of that fluid. In the case of leucocythæmia splenica there was distinct enlargement of the spleen, and white corpuscles were found in undue proportion in the blood, and there was great paleness and tendency to vomiting. Under the use of phosphorus there was decided improvement in the symptoms, the spleen became smaller, and there was return of colour and strength.—*The Practitioner*, January, 1875.

On the Employment of Cold Baths in Cerebral Rheumatism. By Dr. Maurice Raynaud, of the Hôpital Lariboisière, Paris.—Dr. Raynaud, while admitting the extreme simplicity of the treatment of acute rheumatism and other feverish affections by cold water, advises extreme care in resorting to this kind of medication. In his own experience he has seen several cases where the results were unsuccessful, but nevertheless he entertains a favorable opinion of the plan. After showing that the idea of employing cold in acute articular rheumatism is a rather old one, and that local cold applications have been frequently practised in this disease, he still draws a

wide distinction between such treatment and the immersion of the whole body in cold water. But having read a memoir published by a colleague (Dr. Huchard) who had successfully repeated the experiments made by Dr. Wilson Fox in England on the use of cold baths in inflammatory and febrile diseases, he determined to try the effect of such treatment himself. He accordingly adopted the plan in the case of a patient, of robust constitution, who was attacked with acute rheumatism, and in whom cerebral symptoms denoting meningitis supervened. The case is given at length, and a tabular view represents the progress of the case, with the rise and fall of the temperature, the rate of the pulse, and the dates and hours of the several events. The patient recovered completely, and the fall of the temperature and the improvement of the symptoms appeared to follow regularly the use of the cold bath. In remarking upon the case, Dr. Raynaud admits that the bath was not the only agent employed, for he had practised a copious bleeding, of about a quart of blood, to which he attributes very great efficacy. He also enters carefully into the question whether his case was really one of meningeal inflammation, but he shows that it was of that nature, and he observes that cerebral rheumatism is such a fatal disease and runs such a rapid conrse that the promptness of action of the cold immersion is one of its chief recommendations. His plan of treatment differs somewhat from that of English practitioners, for he plunges the patient at once into water cooled with ice, instead of gradually cooling it while the patients are immersed.—*Journal de Thérapeutique*, Nov. 25, 1874.

On the Therapeutical Action of Apomorphia. By Dr. Dujardin-Beaumetz, of Paris.—Apomorphia has been lately employed, especially in the form of hypodermic injection, as an emetic, and Dr. Dujardin-Beaumetz gives the result of his experience as to its use in the Hôpital de la Pitié in Paris. It may be mentioned that apomorphia was first distinctly described in 1869 by Matthiesen and Wright in England, and that these two gentlemen recognised its emetic properties. There is a very great resemblance between morphia and apomorphia in their atomic constitution, the latter being, in fact, morphia which has lost one equivalent of water. Apomorphia is prepared by heating morphia with hydrochloric acid in a closed tube, the resulting substance, which represents only 10 or 15 per cent. of the morphia employed, being a white powder, very easily decomposed. It appears that apomorphia may also be obtained by the action of hydrochloric acid on codeia, the resulting substances in this case being chloride of methyle and apomorphia. Professor Gubler has ascertained that there is a great analogy both in composition and in action between apomorphia and the active principle of an American plant of the natural order of Papaveraceæ, namely, *Sanguinaria Canadensis*. The only preparation of apomorphia used in medicine is the hydrochlorate, a salt of difficult solubility in water, and liable to rapid decomposition when dissolved, for which reason it is recommended that fresh solutions of the salt should be employed, or that it should be dissolved either in glycerine or in saccharine solutions.

It appears to be of no importance at what point of the body the hypodermic injection of the hydrochlorate of apomorphia is performed, but in Dr. Dujardin-Beaumetz's experiments the part selected was always the superior extremity and particularly the outside of the forearm. The dose varied from 5 to 10 milligrammes, (a milligramme is $\frac{1}{1000}$ of about 15 grains) and the latter dose was never exceeded. The effect was produced in from five to fifteen minutes after the injection, and the difference in time was in proportion to the quantity of the apomorphia and the peculiar condition of the patients, the action being more rapid when the dose was large, and less prompt in aged than in young persons. Immediately after the puncture Dr. Dujardin-Beaumetz took care to give the patients tepid water, and the emetic effects were produced without pain or any great effort. The patients stated that the emetic effects were less distressing than those caused by ipecacuan or tartarized antimony. Immediately after vomiting, all the patients, without exception, evinced an uncontrollable inclination to sleep, and Dr. Dujardin-Beaumetz attributes this result, so different from that of other emetics, to the resemblance existing between the effects of morphia and those of apomorphia, the former sometimes causing vomiting when injected. He has employed the apomorphia in twenty cases, and he arrives at the conclusion that the hypodermic use of its hydrochlorate is attended by rapid vomiting, and that the action of the drug does not appear to be diminished by use, and that it does not cause any irritation of the mucous membrane of the alimentary canal. He thinks that it ought to take its place in medicin as a useful emetic, more especially if two slight inconveniences in its nature could be obviated, namely, its difficult solubility and the rapid alteration of its solutions.—*Bulletin Général de Thérapeutique*, Oct. 30, 1874.

On the Use of the Cyanides in Acute Articular Rheumatism. By Dr. A. Luton, of Rheims.—Dr. Luton, although admitting the efficacy of colchicum, and of propylamine and trimethylamine in the treatment of acute rheumatism, points out certain inconveniences attending the employment of these remedies, and advocates the use of the cyanides in the disease in question. He was first induced to employ these salts in a case where he was unable to prescribe opium, and where bromide of potassium had failed. The patient was labouring under cerebral symptoms, and Dr. Luton, wishing to administer medicine without his knowledge, had recourse to the cyanide of zinc, which occurs as an inert powder insoluble in water, and easy of administration in any vehicle. Dr. Luton describes the effect produced as magical, for with ten centigrammes of the cyanide (a centigramme is the 100th part of about 15 English grains) refreshing sleep was obtained on the following night. On the daily continuance of the dose the symptoms disappeared as if by enchantment, and the patient was able to walk in a few days. The case, however, was rather one of gout than rheumatism, and at first Dr. Luton regarded the cyanide as a specific for the former malady, and employed it successfully in several cases; but a partial want of success

in some instances, and failure in others, led him to extend the sphere of his observations and to try the effect of the salt on rheumatism. His success was here complete, and it was the more striking in proportion as the affection was more acute. He gives the history of ten cases, in all of which the success was very remarkable, and in one it was found that the temperature was distinctly reduced under the use of the cyanide.

The use of the cyanides in general has not been hitherto well established in medical practice, with the exception, of course, of prussic acid; although it appears that Prof. Brera, of Padua, employed this acid in several inflammations and in rheumatism. Among the cyanides specially employed by Dr. Luton are the cyanide of zinc and the cyanide of potassium. The first is easily taken in pills or suspended in mucilage; it has no taste or smell, and may be given without the patient's knowledge. It seems to be dissolved in the gastric juice. The doses employed by Dr. Luton varied from five to ten, fifteen, and even twenty centigrammes. The cyanide of potassium might, perhaps, be the preferable drug by reason of its more evident action, but its taste is disagreeable, and the form of pill should be preferred for its administration. Dr. Luton has not exceeded the dose of fifteen centigrammes in the day, and sometimes he has been obliged to reduce the dose owing to the supervention of colic and vertigo. The physiological effects of the cyanides in medical doses are not well marked, but in somewhat large doses they produce frontal headache, nausea, a little colic, and sometimes slight diarrhœa; but frequently the stomach is gently stimulated, the appetite is developed, and the digestion is improved. They exercise a certain amount of sedative influence and encourage sleep. In a therapeutical point of view Dr. Luton found that they relieve pain, and also diminish the redness and swelling of the affected parts in rheumatism. The action of the heart and pulse is lowered by their use, as when digitalis is employed, and the urine appears to be influenced in a critical manner, being always turbid when a notable improvement of the symptoms appears. Dr. Luton considers it certain that the cyanides cure acute articular rheumatism in its original form and in its different transformations, and he thinks that they do so by shortening the duration of the disease and diminishing the risks of complications which are peculiar to it. They act rapidly, which is one great recommendation in any drug; the remedy is not disagreeable to take, and it is anodyne.—*Bulletin Général de Thérapeutique*, Jan. 15, 1875.

On the Action of Veratrum Viride upon the Circulation. By H. C. Wood, Junr., M.D., assisted by Joseph Berens, M.D.—It appears to have been positively determined that there are two alkaloids, and only two, in *veratrum viride*, namely, *veratroidia* and *viridia.* It is easy to separate these alkaloids from one another, but it is very difficult, if not impossible, to separate either of them from adherent resin. The resin is inert, and is therefore only a chemical and not a physiological impurity, diminishing the activity of the alkaloid, which must be given in 20 per cent. larger than the

prescribed doses, because that is the percentage proportion of resin. In the experiments made upon dogs and cats by Drs. Wood and Berens it was found that the direct action of veratroidia was to lower the arterial pressure, and it was further proved that this action depended upon the influence of the alkaloid on the heart itself, and was not exerted through the nervous centres, for some of the experiments were made after the division of the spinal cord and the pneumogastric nerves. From other experiments, however, it would seem that the retardation of the pulse by veratroidia is due to a stimulant effect upon the inhibitory nerves of the heart, namely, the branches of the last-named nerve, for in some cases where the heart was brought to a state of rest by the influence of veratroidia on these nerves the organ was rendered active again by their division, that is to say, by the removal of the inhibitory influence. The conclusions arrived at by Drs. Wood and Berens are somewhat anomalous, and require very careful study before their full import can be understood, and the difficulty of understanding them is increased by the fact that the exact influence of the vaso-motor system, of the cardiac branches of the pneumogastric, and of the intra-cardiac ganglia, on the action of the heart, is not yet fully proved. Whatever the cause of the operation of these alkaloids may be, however, it seems certain that both veratroidia and viridia agree in their power of depressing the heart's movements. Veratrum viride has therefore the same effect as depletion, for it withdraws the circulating fluid from the inflamed part almost as directly as venesection, and the drug is consequently very valuable in many sthenic conditions in which depletion is indicated.—*Philadelphia Medical Times.*

On the Physiological and Therapeutical Action of Jaborandi. By M. Albert Robin, of Paris, Dr. Féréol, of Paris, and Mr. J. N. Langley, of Cambridge.—The substance called Jaborandi was first sent to Prof. Gubler, of Paris, by Dr. Coutinho, of Pernambuco. Dr. Gubler has used it at the Hôpital Beaujon, has confirmed the report as to its sialagogue and diaphoretic properties, and has determined its most important botanical characters and therapeutical indications. Dr. Féréol agrees with Dr. Gubler as to the sialagogue property of jaborandi, but thinks that its sudorific action is less marked. In the month of March, 1874, Drs. Gubler and Coutinho published the results they had observed from the use of this drug, but the quantity they then had at their disposal was too small for an extensive series of experiments. Since that time, however, a fresh supply has been obtained, and M. Robin has been entrusted by M. Gubler with the duty of studying more completely its physiological and therapeutical properties. As to the name, it appears that in Brazil the words Iaborandi, Jaborandi, Jamborandi, are used to designate any stimulant, sudorific, and sialagogue plants, and include several kinds of *Pyrethrum* and *Gratiola.* But the plant imported by Dr. Coutinho belongs to the family of Rutaceæ, and is the *Pilocarpus pinnatus.* It is rather rare even in Brazil. It grows in the mountainous districts, far from the sea-shore, and is employed

empirically by the natives for the bites of snakes and for pestilential fevers. The leaves of Jaborandi are oval in shape and of a peculiar smell, and when chewed they are rather acrid, but not bitter. It appears that the active principles of Jaborandi, whatever they may be (for they have not yet been isolated), reside in the leaves, and in less proportion in the bark; they are soluble in water and alcohol and one of them is rather volatile. The preparations hitherto employed have been the infusion of the leaves, that of the small branches bruised, and that of the bark, and also a watery extract and an elixir and syrup. The dose of the leaves, given in infusion, is from two or three to five or six grammes (a gramme is about fifteen grains), and that of the watery extract is about one gramme. It should be given to the patient fasting, otherwise nausea and vomiting might ensue, and it may be administered either in a large dose at once or in divided doses.

The general results are that in a short time the face grows red, the temporal arteries beat with increased force, the patient feels a peculiar hot sensation in the mouth and face, and then salivation begins. Soon afterwards the skin of the forehead becomes moist, and drops of sweat appear on the cheeks and temples; the salivation increases, all the salivary glands being involved, the mouth is filled with an enormous quantity of saliva, and the desire of expectoration is incessant; the sweat covers the face and neck, and afterwards all the body. At the same time there is increased lachrymation and discharge of mucus from the nose, and the glands of the back of the mouth and the trachea and bronchi are all brought into activity. After the termination of the sweating and salivation the patient feels depressed and inclined to sleep. M. Robin has administered the Jaborandi in several cases of a miscellaneous character, such as acute articular rheumatism, albuminuria, bronchitis, lead-palsy; in some patients there supervened painful swellings of the submaxillary and sublingual glands, and, in one case, of the parotid gland. He gave it in ninety cases altogether, some being persons in health and others suffering from illness. Dr. Féréol gave it in the cases of eight patients, and he agrees in some of the conclusions arrived at by Prof. Gubler and M. Robin, but he has not yet arrived at any important results in reference to the therapeutical action of the drug. In this country a small quantity of the alcoholic tincture and of the glycerine solution of the alcoholic extract of Jaborandi have been examined, and Mr. Langley, of Cambridge, has studied its physiological action by experiments on the lower animals. He confirms some of the conclusions already arrived at in Paris, and he finds, moreover, that when injected beneath the skin of the frog it causes tetanic convulsions not unlike those of strychnia, the result being due to its action on the spinal cord. It also diminishes the rate of the pulse, in consequence, as it appears from Mr. Langley's experiments, of stimulation of the inhibitory fibres of the pneumogastric nerve. Mr. Langley concludes that Jaborandi in many points is similar in its action to physostigmin, but has yet distinct characters of its own; that it depresses

the heart's action, and that this property should be held in view by those who employ it in medicine, and that its diaphoretic powers are very peculiar. He states, however, that his researches on the action of the drug are still incomplete.—*Journal de Thérapeutique*, Dec. 19, 1874, Jan. 10 and 25, 1875. *British Medical Journal*, Feb. 10, 1875.

On Various Therapeutical Uses of Calabar Bean, especially in Tic. By Dr. MUNRO, of Cupar-Fife.—Dr. Munro has employed the Calabar bean with very satisfactory results in several cases of tic douloureux, which prevails very much in Cupar, owing apparently to its being rather a low-lying place and subjected to malarious influences. The mode of using the bean in some of the cases was by applying Streatfeild's ophthalmic squares to the eye, but sometimes a solution of the extract was dropped in or applied by a camel's-hair brush. Dr. Munro prefers the use of the squares, as being more convenient, but when a rather powerful effect is required he would suggest that they should be prepared with *physostigmatin*, the active principle (which is also, however, called *physostigmin*, and on the Continent *eserine*.—REPORTER). In one case, where the neuralgic pain had been somewhat relieved by the Calabar bean, but where strong contraction of the pupil had ensued, Dr. Munro, thinking that the pain still remaining might be caused by the contraction, used one third of an atropine square in order to counteract the effect of the bean, and the curious result was observed that the latter was indeed neutralized in its powers, and the neuralgic pain *immediately became more severe*. It should be mentioned that in several of the cases Dr. Munro also employed quinine and iron, and he recommends the use of the Calabar bean chiefly as a prompt remedy for neuralgia, leaving time for the adoption of the usual treatment. In other maladies Dr. Munro has found the bean useful, and he has ascertained that it lowers the action of the heart, reduces febrile heat, and relieves pain. He has used it internally in doses from one sixth to one half of a grain of the extract, and he believes that the doses recommended in the 'British Pharmacopœia' ($\frac{1}{16}$ to $\frac{1}{4}$ of a grain) are too small for an adult. The cases he records in which he has observed its beneficial action were derangement of the heart, in which the pain was relieved and the pulse was lowered; febricula, in which the temperature was reduced and the pulse fell; and bilious remittent fever, in which the temperature was also reduced. Dr. Munro is convinced that in Calabar bean and its antagonist atropia the Pharmacopœia possesses two remedies equally valuable with opium, if not more so.—*British Medical Journal*, Oct. 31, 1874.

On the Use of Strychnia in Atrophy of the Optic Nerve. By Dr. G. C. HARLAN, of Philadelphia.—Dr. Harlan, after mentioning the names of several practitioners who have employed strychnia in amaurosis with success, confirms, from his own experience, the beneficial effects recorded by his predecessors in this disease. In his own cases he sometimes gave the sulphate in solution, and sometimes the alkaloid itself in sugar-coated granules, and he

continued it in gradually increasing doses until decided symptoms appeared or improvement ceased. The maximum daily dose varied from one fourth to one half a grain, and the period of treatment from two to ten weeks. He regards the mode of adminstration as a matter of little moment, the nitrate, the sulphate, the acetate, or the alkaloid itself, being equally efficacious, and the results being the same whether it is injected into the temple or the arm, or given by the stomach, provided the system be brought thoroughly under its influence. As to the condition of the optic nerve to which the use of strychnia is applicable, Dr. Harlan gives no decided opinion, but thinks that the treatment at present rests on empirical grounds, and that the ophthalmoscope furnishes no trustworthy data for deciding whether strychnia is likely to be useful or not. There is most hope of improvement where there is least evidence of anatomical change, but this rule is not without exception, as is proved by one of his cases, where both the previous history and the ophthalmoscopical appearances showed that the condition of the nerve was due to inflammatory action. Another of his cases was what is called "tobacco amaurosis," in which strychnia is supposed to be particularly useful, and it proved so in Dr. Harlan's hands. In hysterical amblyopia he tried the alkaloid only once, and without success. He gives the record of five cases, in which there was partial success in four and no improvement at all in one.—*Philadelphia Medical Times,* December 26, 1874.

On the Action of Hydrate of Croton-Chloral on Megrim. By Dr. Sydney Ringer.—Under the term megrim (which is a corruption of the word hemicrania) Dr. Ringer includes the affections commonly called sick headache, bilious headache, nervous sick headache, and hemicrania. The most characteristic and commonest symptoms of megrim are headache and sickness, but in a typical case, according to Dr. Ringer, there is also a peculiar affection of the sight at the onset of the attack, followed by perversion of the sense of touch and of the muscular sense in the arms and legs, disordered speech and defective ideation, and, lastly, headache and nausea. The seat of the affection appears to be in the nervous centres, probably in the optic thalami, but the frequency and severity of the attacks both depend on peripheral exciting causes due to the stomach, intestines, liver, womb, &c. The successful treatment of megrim depends less on changes to be effected in the disordered nervous centres than on the removal of the exciting cause, whatever that may be. The pain of megrim is situated in the fifth nerve, and, considering how closely megrim is allied to neuralgia and how useful hydrate of croton-chloral is in facial neuralgia, Dr. Ringer has been induced to try this remedy for the seizures of megrim, and he indicates the kind of case in which it would be found useful. The patient may be a female, subject for years to sick headache, which is rendered much more severe by some great trouble, or by fatigue, flooding, change of life, &c. The sight is affected, there is nausea, and even severe vomiting, the mind is excitable and irritable, and the skin very tender. Five grains of croton-chloral every three

hours, or even oftener, will give considerable relief in most cases. The drug does not entirely free the patient from her attacks, but in one or two days the pain ceases to be continuous, the intervals between the paroxysms are lengthened, and in some cases a week's treatment suffices to bring back the headache to its original type of an attack once in three or four weeks. Dr. Ringer has also found that the croton-chloral will relieve the slight attacks experienced by some delicate and nervous women after any slight fatigue or excitement.—*British Medical Journal*, Nov. 21, 1874.

On the Local Use of Liquor Ferri Perchloridi in Cancerous Ulcerations of the Uterus. By Dr. C. J. Gibb, of Newcastle-on-Tyne.—After a few remarks on the unsatisfactory results of treatment in cancerous diseases of the uterus, Dr. Gibb states that he was induced to employ the solution of the perchloride of iron in such cases from observing its beneficial action in an obstinate case of menorrhagia, arising from enlarged vascular granulations in the uterine cavity. He gives the history of four cases in which the application of the solution was more or less useful, but he draws a distinction, as to the chances of success, between the cases where the cancer is hard and embraces the whole of the uterus and those where the disease is epitheliomatous, spreading over the vagina and throwing out towards the surface exuberant vascular fungoid granulations. In the latter Dr. Gibb thinks that the applications of cotton wool soaked in the solution of iron clear away the greater part of the diseased growth, allow reparative efforts to be made by the comparatively healthy structures underneath, and hasten cicatrization. When the disease is purely epithelial, and chronic, and rodent in character, and confined to the surface, the treatment described has done most good, and appears to Dr. Gibb to cure even bad cases. The application rarely causes pain, except where the solution has accidentally flowed over the adjacent parts, which have been thereby blistered and painfully excoriated. He therefore takes care to limit the application to the diseased part alone. He has always used the strongest pharmacopœial solution undiluted, as he wishes to secure a caustic action; at first he applied it on a piece of sponge or lint, but finally he found cotton-wool to answer best, as this sucks up any quantity that may be required, parts with it easily, and can be moulded into any form, so as to fill a cavity or cover over and adhere to any growth.—*British Medical Journal*, Feb. 13, 1875.

On the Action of Eucalyptus globulus. By Dr. Hermann Schläger, of Göttingen, and Dr. Mees, of Leiden.—Dr. Schläger has investigated the question as to the power exercised by eucalyptol when injected beneath the skin, in lowering the temperature of the body. He found that there was, especially in dogs, an elevation instead of a fall in the temperature, but that the former result did not ensue when the eucalyptol was given by the mouth instead of by subcutaneous injection. When the decoction of eucalyptus was injected into the jugular vein of dogs the pressure of the blood sank in the mercurial manometer fixed in the arteries, and the action of the

heart was retarded, whether the animal was simply tied up, or was narcotized by opium, or was curarized. Other experiments made by Dr. Schläger seemed to show that when eucalyptol passes over into the blood a direct action is caused on the musculo-motor apparatus of the heart. Dr. Mees has studied the influence of eucalyptol on the putrefactive process, with a mixture of flesh, gum, and water. He found that when the mixture was combined with a certain amount of eucalyptol it preserved its smell and reaction, but when uncombined it exhibited in three days the usual indications of putrefaction. Another experiment convinced Dr. Mees that eucalyptol arrested the development of the *Bacterium Termo* in solutions containing sugar and various salts even more certainly than hydrochlorate of quinia.—*Schmidt's Jahrbücher der Gesammten Medicin*, January, 1875.

On a Peculiar Effect of Chloral Hydrate. By Dr. P. Björnström, of Upsala. — Dr. Björnström has noticed, in persons with an excitable vascular system who have been taking chloral hydrate for some time, a disposition to congestion in the head, which may be developed from very slight causes, as, for instance, from taking small quantities of alcoholic drinks which, under other circumstances, would produce no such effect. He has observed in himself and in other persons that after the use of chloral hydrate for several nights in succession the least portion of spirituous liquors caused a livid redness over the whole of the face, in the nape of the neck and the neck itself, and even in the breast and arms, often with oppression of the brain and palpitation of the heart; but when the chloral was no longer taken, the same or even larger doses of spirits produced no such effects. The same results followed in the case of other warm drinks, such as hot soups, coffee and tea, and there was also mental excitement, but especially in those persons who had a tendency to congestion. Dr. Björnström suggests that the increased tendency to this congestion may depend upon the effect of chloral in diminishing the tone of the vessels or in paralysing to a certain extent the vaso-motor nerves. In proof of his statements, and as a caution in the use of chloral hydrate, two cases of patients are mentioned who had been taking the drug for some months, and in whom a small quantity of beer caused congestion of the brain, palpitation of the heart, and acceleration of the pulse.—*Schmidt's Jahrbücher der Gesammten Medicin*, January, 1875.

On the Use of Copaiba as a Diuretic. By E. L. Dixon.—Mr. Dixon's attention was drawn to the virtues of copaiba as a diuretic from a suggestion of Dr. Wilks, and he records three cases in which the drug in question appears to have been employed very beneficially. It seemed to act by increasing the amount of the urinary water, for the specific gravity fell as the amount of the urine increased. Mr. Dixon's first case was one of ascites from cirrhosis of the liver; the second, of disease of the heart, with emphysema and œdema pulmonum and anasarca; and the third, of disease of the heart, with albuminuria, ascites, and anasarca. In all the cases

there was a marked improvement in the symptoms, and in all the quantity of urine was increased. In one case, not only did the quantity of urine increase, but the albumen it formerly contained absolutely diminished.—*The Practitioner*, February, 1875.

On the Employment of Trimethylamine in Rheumatism and Gout. By W. H. Spencer, A.M., M.B., of Bristol.—It will be remembered that trimethylamine and propylamine have lately been employed with much success in France in the treatment of rheumatism, and the results have been recorded by several writers, especially by Dr. Dujardin-Beaumetz. Dr. Spencer has adopted this plan, and he states that he has treated almost all the cases of rheumatism, and some of gout, that have come in his way at the Bristol Infirmary during the past eighteen months, with trimethylamine in one or other of its forms. Dr. Spencer prefaces his clinical record with an interesting *résumé* of the chemical history and properties of trimethylamine, which have been hitherto but little known to the profession. To state the matter as briefly as possible, the two substances, propylamine and trimethylamine, are what are called *isomeric bodies*, that is to say, they have the same number of atoms, but differently arranged in each, and the properties of each body are distinct. Both are, in fact, *ammonias*, but in one (propylamine) the nitrogen is combined with *two* atoms of hydrogen and *one* of propyle; while in the other (trimethylamine) the nitrogen is combined with *three* atoms of methyle. As in the case of many other substances formed by the processes of organic chemistry, propylamine and trimethylamine are obtained from many heterogeneous sources, and while the former has been prepared from narcotine and from codeia, the latter has been extracted from herring-brine, and also from cod-liver oil, human urine, putrid calf's blood, guano, ergotine, and from *Chenopodium vulvaria* and numerous other plants. Dr. Spencer points out the chemical and physical distinctions between propylamine and trimethylamine, and recommends that the latter alone should be employed in medicine, and he gives formulæ for its preparation. The source is always herring-brine, which is treated with caustic lime or potash, and the result neutralized by hydrochloric acid. Trimethylamine is employed either as a liquid, in doses of from four to eight minims, with peppermint water or other aromatics to disguise the unpleasant taste, or as a salt (chloride of trimethylamine) in the form of pill. Dr. Spencer, in endeavouring to determine the cause of the beneficial operation of this substance, is inclined to refer the effect to its alkaline nature, the treatment of gout and rheumatism by alkalies being of well-established efficacy. "If," he argues, "we have, in rheumatism and gout, arrest of the retrograde metamorphoses of the nitrogenous constituents of the tissues, with accumulation of acid products belonging to the series represented by xanthin and lactic acid at one end and urea and carbonic acid at the other, it is easy to conceive that in exhibiting such a substance as we know trimethylamine chemically to be, we furnish complete conditions for the transformation of these acid products to the state of amides and

their further elimination from the body in proper form." Continental observers have stated that under the influence of trimethylamine the amount of urine excreted is increased, while the amount of urea excreted is diminished; but it has also been observed that a sudden and inexplicable increase in the amount of urea excreted is liable to occur during the use of trimethylamine. In one of Dr. Spencer's cases treated with this drug, and in which the quantity of urine secreted and the amount of urea found every day were carefully noted, there was a decided increase in both, the urine being almost trebled in quantity and the urea more than doubled.—*The Practitioner*, February and March, 1875.

REPORT ON MIDWIFERY, DISEASES OF WOMEN, AND DISEASES OF CHILDREN.

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Midwifery.

1. *A Remarkable Case of Pregnancy in a One-horned Uterus.* By Dr. Moldenhauer ('Archiv für Gynæk.,' Band vii, Heft 1, 1874).
2. *Chemical Examination of the Fluid in Hydatidiform Degeneration of the Ovum.* By Gschleiden ('Archiv für Gynæk.,' Band vi, Heft 2).
3. *A Case of Transfusion of Non-defibrinated Blood.* By M. Blondeau ('Archives de Tocologie,' December, 1874).
4. *Kypho-scoliotic and Transversely Narrowed Pelvis; Cæsarian Section; Recovery of Mother and Child.* By Dr. E. Martin, ('Berlin. Klin. Wochenschr.,' 51, 1874).
5. *Hypodermic Injection of Ergot in Post-partum Hæmorrhage.* By Dr. P. C. Williams ('Trans. Med.-Chir. Soc. of Maryland').
6. *Post-mortem Cæsarian Section in a Case of Eclampsia, with enormous Swelling of the Tongue.* By Dr. Bailly ('Archives de Tocologie,' January, 1875).
7. *On the Condition of the Mouth and Teeth during Pregnancy.* By Oakley Coles, L.D.S. R.C.S.
8. *Exfoliation of the Mucous Membrane of the Bladder in a Case of Retroflexion of the Gravid Uterus.* By Dr. S. Brandeis ('Archiv für Gynæk.,' Band vii, Heft 2).
9. *Microscopical Researches on the Composition of the Lochia.* By Carl Rokitansky ('Centralblatt für Med. Wissenschaft,' No. 32, 1874).
10. *On Eclampsia during Pregnancy, Labour and the Puerperal*

State. By Dr. Cohen ('Archiv für Gynæk.,' Band vi, Heft 1).
11. *On the Study of Acute Ulcerative Endocarditis in the Puerperal State.* By Dr. Olshausen ('Archiv für Gynæk.,' Band vii, Heft 2).
12. *Deformities of the Pelvis.* By Dr. Adolphe Pinard (Thèse de Paris, 1874).
13. *On the Prevention and Treatment of Puerperal Diseases.* By Dr. Wm. Goodell (American 'Obstetrical Journal,' July and August, 1874).
14. *On Compression of the Aorta in grave Hæmorrhage after Delivery.* By Dr. Léon Gros ('Bulletin Générale de Thérapeutique,' January, 1875).

1. A woman, aged 27, was delivered of her first child; the placenta did not follow; attempts were made, unsuccessfully, to remove it; as there was no hæmorrhage, ergot was given and the patient was kept quiet; peritonitis set in, and the patient died in a few days. At the autopsy the bladder and vagina were found normal; the uterus was enlarged; the upper part was expanded into a sac, in which was an aperture, the cause of the peritonitis. There was no trace of the uterine appendages on the right side; the uterine appendages of the left side showed traces of old inflammation; the fallopian tube was bent, and its canal impermeable; the ovary was covered with small cysts, and no recent corpus luteum could be seen. On the right side the ligament, the ovary, and the fallopian tube, were wanting, but in a fold of the peritoneum, near the external os, a well-developed ovary was found, with a recent corpus luteum; nearer the middle line a quite solid body was perceived, which on microscopical examination was found to be a rudimentary uterine horn. The case must be considered as one of pregnancy in a one-horned uterus, with arrested development of the other horn and external migration of the ovum, and rupture of the uterine wall in the second half of pregnancy.

2. Gschleiden has made a chemical examination of the fluid contained in the cysts of two such moles. The fluid varies in consistence according to the size of the cyst it comes from; from the smaller cyst it is thicker than from the larger ones; from the smaller about 29 parts per 1000 was solid matter, from the larger 17 per 1000. The solid matter was albumen, mucin, inorganic salts, and phosphates; no trace of fibrinogenous substance, paralbumin, or sugar, was able to be detected. Leucin and tyrosin were found in the fluid. The quantity of solid matter was increased with the age of the mole; in one, four months old, it was 19 parts per 1000; in the other, five months old, 26 per 1000. The quantity of mucin diminished with the age of the mole, whilst the albumen increased. In amniotic fluid the quantity of solid matter per 1000 lessens with each month. Vogt found at the fourth month about 20 parts per 1000, at the sixth about 9·7 per 1000. Scherer found at term 8·5 per 1000.

Neither leucin nor tyrosin was found in two specimens of amniotic fluid examined.

3. M. Blondeau related to the Société de Thérapeutique de Paris at its November meeting the particulars of a case of transfusion of non-defibrinated blood, which appears to have succeeded so far as the immediate results were concerned. The patient had had several miscarriages, in all of which she had lost much blood. She was again four and a half months pregnant, and had severe epistaxis, and was threatened with miscarriage. Plugging and the injection of perchloride of iron being useless, and the patient being in extremis, sixty-five grammes of non-defibrinated blood were injected, with the best results. Consciousness returned, the pulse fell, the body became warmer, and there was no recurrence of epistaxis.

The same evening the patient miscarried, but the placenta was retained, and as it could not be removed, in spite of various measures employed, ergot was given, and the patient was left. There was no hæmorrhage. For six days the patient went on well, without fever, hæmorrhage, or any kind of suffering. Then the afterbirth came away without hæmorrhage, but the patient was taken with shivering, enlargement of the abdomen, and fever, and succumbed of true puerperal fever.

4. Dr. E. Martin related to the Gynæcological Society of Berlin a case of the above. The woman was a primipara, aged 30. The pelvic outlet was much narrowed transversely; there was only room for three fingers between the tuberosities of the ischia (about $1\frac{1}{2}$ inches). The operation was performed as is usual, the uterine wound was $4\frac{1}{2}$ inches long, and the severe hæmorrhage was only stopped after the edges had been brought together by the catgut sutures. The abdominal wound was closed by wire sutures and a compress dipped in solution of carbolic acid applied. The patient was up on the seventeenth day, left the hospital on the twentieth. The child did well.

5. Dr. P. C. Williams, of Baltimore, in a paper read before the Medico-Chirurgical Faculty of Maryland, 1874, speaks very favorably of this remedy. He relates three cases in which he tried it; in two of these it was not employed till after other means had proved useless. The effect of the hypodermic injection was almost instantaneous, and it was permanent. The fluid was injected into the inside of the thigh, and in none of the cases did an abscess form; about half a drachm of the fluid extract was injected.

6. This is a singular and distressing case. The patient was a strong healthy woman, who had had three children previously with perfectly satisfactory labours. She had gone eight months with a fresh pregnancy when she was suddenly attacked with symptoms of eclampsia. On the occurrence of the second attack Dr. Bailly was sent for, and he and Dr. Dupuy, who also saw the patient, found a quantity of blood flowing from the mouth. This came from a wound made in the under side of the tongue by the teeth during the fit. The hæmorrhage was venous, and was not well controlled even by perchloride of iron; but the tongue, though a little swollen, could be perfectly returned within the dental arch, from which,

nevertheless, it seems to have had a tendency to escape and appear externally. There was no symptom of labour, and as the patient seemed better Dr. Bailly went to procure instruments and assistance in order to enable him to deliver promptly in the event of further eclamptic symptoms supervening. The patient spoke and referred her trouble to her neck, when suddenly she was seized afresh and died. When Dr. Bailly returned he found the tongue enormously enlarged and violet coloured; it could not be returned within the mouth. Thinking he heard sounds of the fœtal heart, Dr. Bailly, with the husband's consent, performed Cæsarian section, but the child was dead, and therefore could not be revived.

Inquiry of those present when the patient died seemed to prove to Dr. Bailly that the patient died from suffocation, and not during a fit, and the prodigious size of the tongue seemed, in his judgment, amply to confirm this view. Whether the enlargement of the tongue was due to simple acute congestion or infiltration of blood was, unfortunately, not determined. Possibly laryngotomy might have averted the fatal issue, but seeing the serious condition of the patient Dr. Bailly does not seem to have been of opinion that it would have ultimately succeeded.

One interesting remark respecting the operation of Cæsarian section is made by Dr. Bailly—it is that the tonicity of the uterine muscle was absolutely extinct when he operated, the time between the death of the patient and the performance of the operation being from fifteen to eighteen minutes. The uterine tissue was quite flaccid, and did not shrink on being cut. If this observation should be confirmed it would have an interesting bearing on the subject of spontaneous post-mortem delivery.

7. Mr. Oakley Coles has presented in this short memoir, which was read before the Odontological Society, an interesting and instructive account of the condition of the mouth and teeth during pregnancy.

The author first gives an account of the varieties of caries occurring during pregnancy. One peculiar change is characterised by marked fatty degeneration. Mr. Coles likens to osteomalacia the softening which some teeth may undergo during the pregnant state. As regards the condition of the gums, Mr. Coles states that so far as his observations have gone he has failed to discover anything peculiar to pregnancy. The oral and buccal secretions are stated to be much changed. Mr. Coles states that the contents of the mouth are found to be acid in a large majority of cases, and especially in first pregnancies, if test papers are applied early in the morning. There is also a great flow of saliva, and this he attributes to the acid matter regurgitated from the stomach during the troublesome morning sickness of pregnancy. Mr. Coles concludes that the great prevalence of caries in pregnant women is due in a very large measure to the acid conditions that obtain in the mouth.

As regards neuralgia, the author thinks it more likely to occur in first than in subsequent pregnancies, and he truly points out that the blood condition of the pregnant is that which in the non-

pregnant seems to be most favorable to the occurrence of neuralgia, viz. a condition in which the quantity of white corpuscles and fibrine is increased. This condition of blood favours the tendency to thrombosis, and the author might, perhaps, have laid more stress upon this as a cause of carious and other nutritional changes in the teeth.

Mr. Coles thinks oatmeal especially serviceable to pregnant women; he advises correction of obvious derangements of health, and special treatment for the teeth, which the dentist should apply.

8. The author was called to a woman, aged 36, who had suffered for three days from retention of urine. She had had three children. The bladder was found reaching to two inches above the umbilicus; a gum-elastic catheter was passed, and a large quantity of strongly ammoniacal muddy fluid was drawn off; a catheter was left in the bladder. By vaginal examination the cervix was soft, and pressed against the symphysis; the posterior part of the pelvis was fitted with a hard, immovable, and semi-globular tumour, separated from the cervix uteri by a furrow. As the woman said she was regular and last menstruated two weeks ago, the swelling was supposed to be a retro-uterine hæmatocele. At a second examination a few days after, the nature of the swelling was ascertained to be that of the pregnant uterus. The woman was put in the knee-elbow position and the uterus restored to its position; the patient was now able to micturate with only a slight pain. Four days after, Dr. Brandeis was hastily called, and found she had been seized with severe pain, and that something was being expelled from the genital organs. This was found to be a complete cast of the mucous membrane of the bladder, thickly covered with crystals of the urinary salts. The patient recovered, and was delivered of a healthy child.

9. The observations which were made on twenty-five lying-in women gave the following results:

The number of red globules diminished from the first day. Purulent globules, few at first and disposed in small masses, became multiplied, but this fact was not constant. There was intense desquamation of pavement epithelium.

Rokitansky inquired into the question whether the elements which have been described under the name of Trichomonas vulvæ might not be constituted by the vibratile epithelial cellules coming from the uterus. The results relative to the presence of cylindrical epithelial cells, masses of granulations, pigment, fat, and mucus, agree with those of previous observers. Granulations and club-shaped bodies endowed with movement were observed in the lochial discharges of lying-in women, whatever was their state of health. Besides these elements (bacteriæ?) were found in the leucorrheal secretions of women, who, not being in the puerperal condition, were affected with uterine diseases such as catarrh, chronic metritis, carcinoma. The inoculations made on rabbits with the lochial secretion gave no positive results, in the sense that the only manifestations observed consisted in local suppurations.

10. The author recognises two forms of eclampsia during labour,

which are different in their prodromata, course, treatment, and pathogeny. The one he calls uterine eclampsia, the other cerebral eclampsia. These, which he considers as due to irritation of the sympathetic nerve system, causing systolic anæmia of the brain, are distinct from epilepsy, which is dependent on the cerebro-spinal system, a diastolic hyperæmia of the brain. Uterine eclampsia is either premature or mature. The eclampsia uterina prematura occurs about the sixth month of pregnancy, and is due to the sinking down of the fœtus upon the lower segment of the uterus. The eclampsia uterina matura may come on at any time during labour. The eclampsia parturientum cerebralis is due to some cerebral lesion, and at first there are no symptoms of uterine activity, and this may occur at any period of pregnancy. The author then gives a number of cases illustrating this arrangement, and then speaks of the differential diagnosis of the different forms of eclampsia and epilepsy. After detailing the treatment of the various forms and of epilepsy, the author gives the pathogeny of the affections, and concludes by saying that the one, eclampsia, is due to irritation of the sympathetic, the other, epilepsy, to irritation of the cerebro-spinal system.

11. The author, after relating in detail two cases, arranges in a tabular form nine cases which he has collected from various sources. The age of the patients affected was high; out of ten cases only two were under thirty years of age, the remaining eight were between thirty-three and forty. In twenty-two cases of ulcerative endocarditis, in patients of both sexes, thirteeen were between twenty and thirty years of age, so that an age over thirty appears to predispose to the affection in the puerperal state; also a previous disease of the endocardium predisposes to the acute ulcerative process in the puerperal state. The duration of the disease varies from one and a half to twenty-eight days. From the symptoms the affection appears to be due to an infection of the blood, some septic material being carried into the circulation. It is most probable that the opportunity of such absorption is afforded by the lacerations which so frequently are met with in the genital psssages after labour. If this is so it is not clear why the affection, in contrast with puerperal septicæmia, is often so late in manifesting itself. It is also to be noted that septicæmia and acute ulcerative endocarditis have not been hitherto observed in the same individual. The inner layer of the uterus in almost all the cases was healthy. The author concludes that malignant puerperal ulcerative endocarditis is a disease *sui generis*, the causal connection of which with other puerperal affections we are as yet unacquainted with.

12. Dr. Pinard, a distinguished graduate of Paris, has devoted some time to the study of pelvic deformities, and his researches are embodied in an excellent monograph on the subject, entitled 'Les Vices de Conformation du Bassin, étudié au point de vue de la forme et des diamètres antéro-postérieurs,' Paris, 1874.

In the introduction Dr. Pinard shows how unsatisfactory are all the pelvimeters hitherto invented, and implies that, although not

perfect, pelvimetry by means of the hand yields much the most satisfactory and reliable results.

The work is divided into two parts; the first being devoted to a critical study of manual mensuration; the second, which constitutes the original part of the work, sets forth the author's researches made on 100 pelves, deformed and normal, of which illustrations are given.

Dr. Pinard distinctly states that he does not propose inquiring into all pelvic measurements, but solely into that which is the most important of them all, viz. that of antero-posterior diameters. The author's observations were made on pelves deformed by rickets, by contraction, by coxo-femoral luxation, by kyphosis, and pelves normal and faulty by excessive amplitude. He has set aside osteomalacic pelves, obliquely ovate pelves, and rachitic pelves with lateral deviations of the sacrum, because he believes these would vitiate his researches for reasons which are given. He has selected an excellent method for illustrating his researches in giving full-sized graphic representations of pelves, showing exactly that which he truly says is the most important point for us to know, viz. *the minimum antero-posterior diameter of the superior inlet.*

The monograph is one which well deserves study, and as we cannot reproduce here that which constitutes one of its chief attractions, the graphic illustrations, we must recommend those interested in pelvimetry to look into the work for themselves.

13. Dr. Goodell here sets forth his views and experience on the method of treatment of puerperal women in vogue at the Preston Retreat (Philadelphia), a small lying-in hospital for reputable married women. The hospital contains four wards, each furnished with five beds, four of which are generally occupied at one time, the wards being invariably used in rotation and in such a manner that one ward in its turn stands idle for two or three weeks. Carbolic acid is freely used for cleansing the wards. The nurses wear only such clothing as can be washed, and are required to be scrupulously careful as regards their personal cleanliness. Great care is taken with all bedding, linen, &c. The diet is liberal, and vegetables are largely employed.

When a patient falls in labour her bowels are emptied by injection, the "waters" are ruptured early, and the patient is never allowed to be long in the second stage of labour, either the vectis or forceps being used. The placenta is removed by Crede's method (expression). The child is not bandaged, but the mother is, and a cylindrical compress is applied under the bandage in the hollow just beyond the fundus of the womb. A drachm of fluid extract of ergot is invariably given as soon as the head presses on the perinæum. The binder is only worn for one day, being removed for good the morning following the day of labour, and on that day and each day following the patient has to sit in a chair while her bed is being made. No woman is allowed to suffer from after-pains, morphia being used until relief is obtained. In stubborn cases of after-pains Dr. Goodell has found ten grains of quinine every six hours of great

service, a plan recommended by Dr. Fordyce Barker, one of the ablest American physicians. If the lochia should become offensive the patient is required to "slip into a chair three or four times a day," which usually corrects the fœtor, failing which Condy's fluid is used. Should any untoward symptoms arise, quinine in six- to ten-grain doses is given frequently; morphia is used for pain, and in addition for abdominal pain iodine is painted over the abdomen and a poultice applied. "The canonical purge on the third day is dispensed with."

Dr. Goodell enters into an explanation of the rationale of the foregoing practice, which certainly, whatever may be the explanation of it, seems to have been attended by very gratifying success. In 756 cases there were but six deaths.

14. Dr. Léon Gros advocates this method of controlling post-partum hæmorrhage. He gives an historical account of the practice, showing that it was approved by Baudelocque, Siebold, and others. Ulsamer, of Wurzburg, seems to have been the first to write about this plan of treatment in 'Beiträge zur Natur und Keilkunde,' Wurzburg, 1825, from which time it appears to have been put into use both in France and Germany, though it is doubtful if it ever became very popular. It certainly deserves to be borne in mind by every accoucheur as a ready method of preventing extreme loss; and as one which, if not of itself successful in permanently arresting uterine hæmorrhage, may enable the practitioner to devise and prepare other plans, *e. g.* injection of astringents, &c.

Diseases of Women.

1. *The Diagnosis of Cystic Myoma of the Uterus and its Intra-peritoneal Enucleation; a new method of Operation.* By Dr. Spiegelberg ('Archiv für Gynæk.,' Band vi, Heft 3, 1874).
2. *On Sarcoma of the Uterus.* By Dr. Kunert ('Archiv für Gynæk.,' Band vi, Heft 1, 1874).
3. *Solid Tumours of the Ovary.* By Dr. Leopold ('Archiv für Gynæk.,' Band vi, Heft 2).
4. *Hæmatometra in a Uterus Bicornis, with complete absence of the Vagina.* By Dr. A. Bidder ('Berlin. Klinische Wochensch.,' 46, 1874).
5. *On the Natural and Artificial Elimination of Uterine Fibroids.* By Dr. Männel, of Dresden ('Vierteljahrsch. für die Prakt. Heilk.,' 1874).
6. *Successful Operation for the Removal of a large Fibro-Myoma from the Fundus Uteri.* By Mr. Lawson Tait (Royal Med. Chir. Soc., Oct., 1874).
7. *On the Treatment of Fibro-Myomata of the Uterus by Subcutaneous Injections of Ergotine.* By Dr. Hildebrant ('Zeitschrift für Prakt. Med.,' No. 45, 1874).
8. *On Ovarian Compression in Hysterical Crises.* By M. Charcot ('Gaz. Méd. de Paris').

9. *On the Treatment of Vaginal Discharges.* By M. GUIBOUT (Lecture at the Hôpital St. Louis, 'Gazette des Hôpitaux,' Feb., 1875).
10. *Extirpation of a Fibro-Cystic Tumour of the Uterus, together with the Uterus and its Appendages.* By E. H. TRENHOLME, M.D., Professor of Midwifery, &c., Montreal. ('Lancet,' November 14, 1874.)
11. *Extirpation of the Uterus for Tumours.* By Dr. WOOD ('St. Louis Medical Journal,' Dec., 1874).

1. The author gives in detail the following interesting case. A patient, aged 28, was supposed to be suffering from ovarian disease, and she was operated upon. The ovaries were not affected; a large tumour reaching four inches above the umbilicus was found to spring from the posterior aspect of the uterus from about one inch below the fundus to the insertion of the vagina; it was covered by the uterine peritoneum and the posterior layers of the broad ligaments. This tumour was punctured and about half a bucketful of dark yellow fluid, which coagulated at once and completely, was removed; the growth was then drawn out, ligatured, and cut off with the knife. As hæmorrhage continued, the remaining portion embedded in the uterine walls was enucleated, leaving a deep hollow the size of the fist. This cavity was closed by drawing its peritoneal edges together by eighteen silk sutures; the ends of these were brought out at the abdominal wound and a drainage tube was passed through the retro-uterine pouch into the vagina; the abdominal wound was then closed. The patient died suddenly on the sixteenth day—no autopsy was permitted—most probably from pulmonary embolism. The diagnosis of cystic tumours of the uterus by physical examination alone is almost impossible. They are frequently taken for ovarian tumours, and thus their nature is only discovered at the operation. The exploratory puncture is the only certain diagnostic test; the rapid and complete coagulation of the fluid reveals its character. Spiegelberg calls this operation intra-peritoneal enucleation with bringing of the peritoneal edges of the cyst together, and thinks it more favorable than removing the whole or a portion of the uterus. He speaks favorably of Marion Sims' system of vaginal drainage in ovariotomy.

2. The author gives the history of six cases which he has watched till the fatal end, and gives a *résumé* of the chief points in the pathology of the affection. Sarcoma of the uterus has not been known to occur before puberty. The progress of the disease is very rapid, and always ends fatally; the alteration in the general condition of the patient is most marked. In fourteen out of thirty cases death followed within a year after the tumour was recognised, in one after two years, in three after three to six years. There is but one case of cure, that réported by Winkel. The diagnosis is generally easy. Carcinoma of the body of the uterus is exceedingly rare, and the consistence of the growth is different. In carcinoma of the cervix the débris of the tumour is found in the discharge. It is

difficult to distinguish a sarcoma from a myoma. The prognosis is bad, only a little more favorable than in carcinoma. With regard to treatment, as a preventive every myoma should at once be removed. As soon as the sarcomatous growth is recognised it should at once be removed; if the tumour is circumscribed it should be cut through with the knife or écraseur. If the growth is diffused it should be taken away with the curette, scoop, finger-nail, &c. In cases where it is impossible to operate fair results are obtained from the repeated injection of a solution of carbolic acid, tincture of iodine, caustic potash, or the actual cautery.

3. Dr. Leopold has arranged in a tabular form fifty-six cases of solid tumor of the ovary; thirteen of these are now published for the first time. His remarks on these are arranged under three heads:—1 the general (including the frequency, character, and size of the tumour, and the age of the patient); 2, the anatomical; 3, some clinical points of importance (menstruation, &c.). Solid tumours of the ovary are very rare—from the facts collected, about 1·5 per cent. of all tumours of the ovary. These tumours retain more or less the natural shape of the ovary, and may thus be distinguished from the fluid tumours, which have an irregularly rounded form; their consistence varies, from being so soft as to give the suspicion of fluid to being as hard as stone. The anatomical relations are almost identical with those of the fluid. Their external envelope is usually thick, and this, doubtless, is of importance, as limiting their too rapid increase of size. Histologically, these tumours may be fibrous, enchondromatous, sarcomatous, or carcinomatous. Enchondroma is very rare. Sarcoma is also said to be very rare. Under clinical remarks Dr. Leopold draws attention to the condition of menstruation in women suffering from cysts or solid tumours of the ovary. We know now of many instances where menstruation has gone on when the two ovaries have been degenerated, and even after they have been removed; from these facts the author inclines to the belief that menstruation has no direct relation with ovulation. Ovariotomy was performed eight times for these solid tumours of the ovary, and only three times successfully. The Cæsarian section was performed once on a woman whose pelvis was narrowed through the presence of a partly ossified fibroma.

4. A patient aged 17½ years, had for the last year suffered from pains recurring every four weeks, but there had been no menstrual discharge. On examination no vaginal orifice existed; a slight depression marked its position; a finger in the rectum felt a catheter in the bladder, separated apparently only by some connective tissue. About three inches from the anus the finger came upon a large elastic body the size of the fist, in the position of the uterus and felt bimanually; it was freely movable; no cervix could be felt. As the diagnosis was not doubtful, it was determined to make or open up the vagina, supposing the malformation was due to its closure by adhesions. This was done by boring with the finger, placed in the depression, in the direction of the pelvic axis; this was accomplished in some places easily, and the uterus was reached at a depth of

about $3\frac{1}{2}$ inches; a small hole was made with the nail and a borer pushed in; a thick tenacious treacly fluid poured out; the opening was enlarged, and the cavity allowed to drain without any pressure being exerted; injections of tepid water were used. The patient did well. Menstruation occurred normally shortly after. Later on the patient was again examined under chloroform; the uterus was soft and elastic; to the left and below the fundus a thick projection was felt, $1\frac{1}{2}$ inch long and $\frac{3}{4}$ inch broad, which sprang directly from the uterine substance; this, from the nature of the case, was probably the other horn of a uterus bicornis; no vaginal portion could be detected, but a small cord-like vagina was found. The vagina had commenced contracting, so that the finger only entered about one inch; a probe then could be passed; attempts to dilate failed. The patient was, however, menstruating normally.

5. The elimination of fibroid tumours may be effected spontaneously in two ways—by disorganization, suppuration, or gangrene of the tumour, or by spontaneous enucleation and expulsion. These two processes have been imitated in medical practice, but the dangers which accompany the first mode of treatment have caused it to be almost abandoned for the second.[1] And this differs in its procedure; the enucleation and extirpation of the tumour may be made at a single sitting, but, with Duncan and Sims, the author prefers, at least in special indications, to imitate nature and divide it into several sittings, separated by variable intervals, but often as long as several weeks, and even several months. Dilate the cervix if it be not dilated, divide the mucous membrane of the uterus when the tumour is immediately submucous, and let the section enlarge of itself; if the incision in the capsule cicatrizes instead of enlarging, or, again, if the tumour is less superficial and approaches nearer the cervix uteri, practise section at the level of the extremity nearest the tumour and begin enucleation at once at the lower part of the wall without dissecting the mucous membrane; complete enucleation at several sittings, and be guided by the general state of the patient, by the greater or less tendency to spontaneous enucleation. Such, in brief, is the practice recommended by the author, and such are the conclusions which he thinks should be drawn from the observations published by gynækologists and analysed in his memoir.

One point on which the author, however, differs from the opinion still generally admitted, at least in France, is the value of partial extirpation of fibroids. This procedure, absolutely rejected by Courty, is, says the author, dangerous, especially on account of the hæmorrhage to which it is liable; nevertheless he recognises one chief indication—the troubles caused by compression of the bladder by the lower part of the fibroid when the complete extirpation of the growth is not practicable. In support of his opinion he quotes some cases followed by cure, one occurring in his own practice, in

[1] We believe that Dr. Greenhalgh in this country has resorted to the first method, and has used and still uses with some success the actual cautery for the purpose. Retzius, we believe, was the first to use the actual cautery in such cases.—*Rep.*

which at two operations at four months' interval he removed a mass of fibroid, and the urinary troubles which necessitated the operation had not recurred at the end of two years; on the other hand, the metrorrhagias thenceforward became more frequent. The memoir ends with a statistical table of enucleations and extirpations of uterine fibroids, continuing the statistics published by West in his work on 'Diseases of Women.'

6. The patient, aged 34, had suffered from a large abdominal tumour of rather rapid growth for five years. Menstruation was profuse, and there were frequent symptoms of pressure upon the pelvic organs. The tumour was central, and reached two inches above the umbilicus, was completely solid, and movable with the uterus. It was removed by an operation, the steps of which were exactly the same as for ovariotomy. The tumour was found to spring from the whole of the fundus uteri, and that part of the organ above the internal os was removed with it. Recovery was rapid and uninterrupted; the clamp came away on the fifteenth day. The tumour weighed eleven pounds, and was an ordinary fibro-myoma.

7. Dr. Hildebrandt has made some observations on this method of treating uterine fibroids, a method which has been recommended by many gynækologists, in particular by Scanzoni, and affirms that he has obtained the best results. In a certain number of cases he has uniformly observed the functional troubles caused by the tumour disappear without the volume of the tumour being influenced. At other times the disappearance of the symptoms coincided with the diminution in volume of the tumour. Only two cases were completely rebellious to this plan of treatment.

According to Hildebrandt, the best conditions for the success of subcutaneous injections of ergotine are met with in those cases in which the tumour is rich in muscular elements, where it is submucous, and where the contractility of the uterus was perfectly intact. Hildebrandt takes care to add some glycerine to the solution of ergotine, to prevent the development of low organisms.

8. M. Charcot admits that in the majority of hysterical crises there exists an aura taking its departure from one of the ovaries, and sometimes from both. This stated, the exercise of strong pressure over the ovary which is the seat of the aura is sufficient to suspend the crisis almost instantaneously. In a late conference at the Salpêtrière, the learned professor showed the efficacy of this treatment in a case of hystero-epilepsy. He several times suspended the crisis at pleasure by exercising pressure on the left ovary. As soon as he ceased to compress the ovary the crisis returned.

To exercise pressure for a sufficiently long time, and thus to cause the abortion of the crisis, Dr. Charcot employs a sort of tourniquet at the Salpêtrière.

Pressure with the fingers is made exactly in the same way as when the iliac artery is compressed. The fingers are, in fact, forced behind the pubis, and the artery is felt beating below them.

9. Dr. Guibout points out that discharges may be due to an

excessive flow of the natural secretions of the part, such as the vulvo-vaginal liquid, vaginal mucus, &c. Morbid secretions from the vagina are purulent, yellow or greenish, when due to inflammation, while those from the uterus are glutinous. Ordinary leucorrhœa is but an exaggeration of the natural secretion, and is often due to ill health.

Dr. Guibout declares injections useless for these vaginal discharges; he employs the tampon, and generally cures his patients in from eight to ten days. Various astringent solutions—nitrate of silver, sulphate of zinc, perchloride of iron, alum—are often employed, but these he has found to be so inconvenient for many reasons that he has completely put them on one side for a solution of tannin, in which he soaks lint. A tampon thus made is inserted every day, and allowed to remain for twenty-four hours. The patient is required to keep in the horizontal position, and to move as little as possible. On the removal of a tampon detersive injections are made before another is inserted. Dr. Guibout attributes benefit to the plugs from their acting as foreign bodies, modifying the vitality of the diseased surfaces and keeping them apart. The astringent aids this.

10. The tumour in this very successful case weighed sixteen pounds, and was adherent to a considerable extent to the adjoining parts, including "an attachment to the bowel to the extent of about ten inches." The operation was difficult, occupying two and a quarter hours in its performance, and at its conclusion there were alarming symptoms of collapse. These, however, under stimulants, passed off, and in spite of vomiting, which appears to have been caused by morphia and relieved by aconite, the patient progressed well and rapidly recovered.

11. In five cases of removal of the womb for fibroids of the organ Dr. Wood reports three successes. There are one or two points of interest in Dr. Wood's method of operating which deserve mention. Dr. Wood secures the vessels by ligatures before dividing the tissues. He also wraps up the intestines in the omentum before allowing them to fall into the pelvis, thus preventing them forming adhesions to the stump. Another point is leaving the ovaries when possible, because in this way it is believed that the patients enjoy better spirits, and are more apt for the pleasures of married life.

Owing to pressure of space, this report is necessarily curtailed, that on diseased children being omitted.

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